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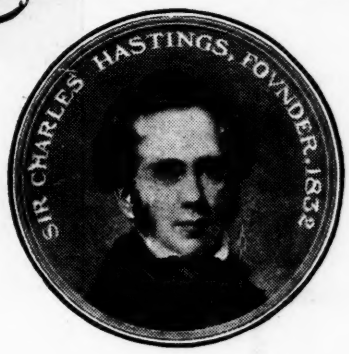

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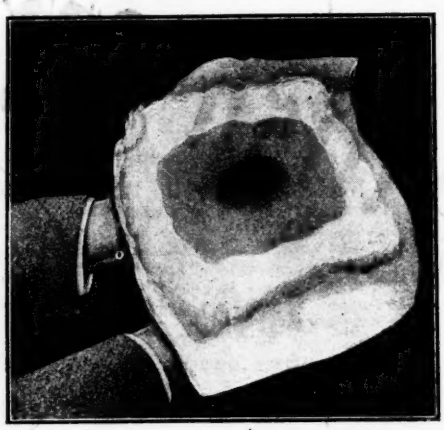

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MEDICINE IN THE CENTURY BEFORE HARVEY.

BY
ARNOLD CHAPLIN,
HARVEIAN LIBRARIAN OF THE ROYAL COLLEGE OF PHYSICIANS OF LONDON.

Mr. President, Fellows, Ladies and Gentlemen,—In obeying the command of your predecessor to deliver the Harveian Oration I desire, Sir, to record my deep sense of the great honour bestowed upon me, and of the diffidence I experience in approaching a task that has occupied the abilities of so many distinguished men in the past. Already two hundred and two Orators have complied with the conditions formulated in Harvey's bequest. The subject has been presented from many aspects, and I should be guilty of an act of presumption were I to suppose that any part of the field had escaped the minute investigation of so many acute intellects.

Harvey, when founding this oration, indicated with great precision its aims and its extent, but the provisions elaborated with such care by the founder have been interpreted by successive orators in various ways. Sometimes the orator has obeyed the injunctions of Harvey more or less literally, while at other times a brief reference to the commands of the founder has sufficed to link them to the subject of the oration. But Harvey desired this oration to be, above all, the medium of an annual exhortation to this College for the express purpose of keeping alive in the minds of the Fellows certain important principles for the guidance of their conduct in private and professional life—principles as worthy of acceptance to-day as they were in the seventeenth century. For this reason I am constrained to call attention to the commands contained in Harvey's bequest, for they form the essence of the creed of all honourable physicians whether ancient or modern.

These commands of Harvey may be considered for a moment with reference to the manner in which they have been modified to meet the needs of modern times. The founder ordained that this oration should be delivered in Latin, but that tongue has not been heard on this occasion since 1864. The Latin language has ceased to be the usual method of communication between the learned, and by common consent that injunction has been allowed to fall into abeyance. The second injunction imposes upon the orator the duty of mentioning by name all the benefactors of the College, of recounting the work they have accomplished, and of exhorting others to emulate them. But here again the lapse of years has swelled the roll of those who have benefited the College to such an extent that the time at the disposal of the orator would hardly permit him to comply with the command. A judicious selection of names, therefore, is all that the orator can attempt in obedience to this act of piety demanded by the founder. The two remaining injunctions, requiring us "to search and study out the secrets of Nature by experiment, and also for the honour of the profession to continue in mutual love and affection," present no difficulty to the orator of the twentieth century, for they comprise the whole duty of the physician in every age in which his life is passed. But when Harvey in 1656 attached to his bequest these two fundamental commands the peculiar condition of political and scientific thought rendered them specially imperative. The experimental method in science was then in its infancy, and many physicians still clung obstinately to the teaching sanctioned through centuries by authority, itself based on insecure foundations. Political and religious feeling ran high in the seventeenth century, and the admonition "to continue in mutual love and affection" became the solemn duty of every man who had the welfare of the College near to his heart. Although no direct evidence exists in support of the contention, it is impossible to believe that some echo of the fierce political and religious passions then raging in England was not heard within the walls of the College. For the College contained in its body men ready to die for the king, and men just as ready to send that king to a shameful death; men to whom the surplice was an abomination, and men who sighed for the days of universal orthodoxy, before King Henry VIII had raised his destroying hand. All honour, then, to this College for the restraint it was able to exercise, so that, while Puritan and Royalist were engaged in a deadly struggle, the Fellows, holding the same divergent opinions, were able to meet and conduct their business with such decorum that history has been unable to record any overt act of enmity. The amount of control needed may be imagined when men like John Bathurst and Laurence Wright, both in Cromwell's service and high in his favour, took their seats at the Comitia side by side with Harvey and Sir Charles Scarburgh, who were no less zealous in the service of their king. May such moderation be an example for all time!

This College, since its foundation in 1518, may be regarded as representative of medical thought in this country, for, with few exceptions, those who have contributed to the advance of the science and art of medicine have been members of its body. But of all those who have conferred honour and distinction upon the College by reason of their additions to our stock of scientific knowledge William Harvey occupies alone the place of honour. His great work, in which he gave for the first time the true explanation of the manner in which the blood circulated, was published to the world in 1628—110 years after the foundation of the College. The appearance of that work marked the beginning of the epoch of modern physiology, and the close of the long period during which the authority of Galen had held undisputed sway. After the publication of the *De Motu Cordis* the scales fell from the eyes, and men broke for ever with the old and inept tradition that had for centuries hindered the advance in scientific thought concerning biology. But however far-reaching the influence of Harvey's great work may have been, it must not be forgotten that it was but one of the epoch-making movements taking place at the same time in other branches of science. For the early years of the seventeenth century witnessed the gathering of the first-fruits from that vast change produced largely by the revival of learning—a change which ended mediaevalism and freed the human mind from the fetters of blind adherence to authority.

In my attempt to comply with the conditions laid down in the bequest of Harvey I propose to direct your attention to a consideration of the history of medicine and science during the first hundred years of the existence of this College, or up to the date of the appearance of Harvey's work. In carrying out that project it will be my duty to indicate briefly the chief causes responsible for the decay of learning during the Middle Ages and the revival of letters in the fifteenth century; for the rebirth of the study of medicine and science I hold to be a part of that wonderful change. I shall then pass to a review of the state of medical and scientific knowledge during the sixteenth century, or up to the appearance of Harvey's work in 1628. Finally, I shall offer some considerations concerning the change wrought in medical and scientific thought during the seventeenth century by the advent of Harvey. In adopting this method I shall be able to select from the roll of the College types of mental activity in illustration of the various phases through which medical thought passed during the period under review.

The Age of Authority.

Until the dawn of the sixteenth century, coeval with the foundation of this College in 1518, it might be asserted that the study of medicine had made little advance in Europe since the Greek period, which closed with the age of Galen. For more than a thousand years, therefore, the world was content with a medical system constructed in ancient times. But it was not medicine and science alone that failed to register any advance, for the study of all branches of learning was involved in the same black night of stagnation and obscurity, and as age succeeded age Europe receded farther and farther into the abyss of ignorance and intellectual sloth. The chief cause for this lamentable state of learning in Europe was political, and really began on the day when Constantine the Great, in 323, spear in hand, followed the invisible heavenly guide in marking out the limits of the city of Constantinople, the future capital of the East. Although the great founder fondly hoped to rule both East and West while established on the fair promontory at the entrance of the Bosphorus, it soon became evident that the two empires must separate and accomplish their destinies in their own appointed ways. By the time that the great Theodosius was carried to his grave in 395 the division of the empires, begun

in 364, was complete, and the Greek influence was confined to territories along the shores of the Bosphorus, the Propontis, and the Eastern Mediterranean, with stately Constantinople as its diadem. As time proceeded, Greek thought, learning, and language became more and more circumscribed within the ever-narrowing limits of the Eastern empire, and the little progress their influence made was entirely in the direction of the east and south. Meanwhile the Western empire in Europe steadily discarded the Greek influence until scarcely any vestige of it remained. To all but a few the Greek tongue in Europe was unknown. Barbarians occupied the throne of the Caesars, and a crude system of theology prescribed the limits of knowledge, beyond which the minds of men were forbidden to venture. From time to time half-hearted attempts were made to revive Greek thought in the West, but the result was transient, and the dark cloud of ignorance and indifference settled down once more.

Yet it must not be supposed that all learning was dead. The classics were still read in the West, but the knowledge they contained was closely guarded by the religious houses, and was the appanage of their inmates alone. The Church soon became aware that its power, both spiritual and temporal, required support that could be obtained only by an appeal to the intellect. In response to this demand, in the eleventh century was laid the first foundation of that system of learning known by the name of "Scholasticism" which has amazed and, perhaps, appalled the intellectual world ever since. In support of the tenets of the Church the aid of metaphysics borrowed from Aristotle was invoked, and a system was reared having for its basis certain principles founded upon the authority of antiquity which it was sacrilege to impugn. For centuries intellectual Europe was engaged in the barren task of commenting upon these principles. They could not be submitted to destructive criticism, for to doubt or to inquire entailed pains and penalties in this world and in the world to come.

The Revival of Learning.

But although Western Europe was unacquainted with the Greek tongue at this time, it was nevertheless possessed of a knowledge of the writings of the Greeks by means of translations into Latin through the medium of Arabian and Syrian translations from the original Greek texts. Through the same tortuous channels the medical writings of the Greeks were brought to mediaeval Europe, were appropriated by the "schoolmen," and were subjected to the empty and narrow methods of the commentators. Meanwhile the Eastern empire was slowly dwindling, and its limits were gradually becoming confined to a small area with Constantinople as its centre. But, as Philelphus has said, in that peerless city the Court and the Church still spoke the language of Aristophanes and Euripides, and the libraries were still stored with the priceless manuscripts of the learning of ancient Greece. Although the Greek tongue was lost to Europe, and although the Eastern empire was becoming every year a more and more circumscribed civilization, two events were about to take place which may be regarded as first steps towards the restoration of the Greek spirit in the West. I refer to the Crusades, and the occupation of Constantinople by the Franks for a period of nearly sixty years. From the fervent pages of Anna Comnena we are made acquainted with the astonishment of the first Crusaders when, with predatory eye, they beheld for the first time the Greek capital in all its luxury and glory. We know also how frightful was the spoliation to which that fair city was subjected by the Franks when, in the name of the Master which both contending parties served, they marched through blood to its capture. It is, however, impossible to suppose that the visits of the Crusaders and the occupation of the city by the Franks did not contribute to the diffusion of the Greek spirit in the West.

The final act which unlocked the treasures of Constantinople and gave to Europe a full knowledge of Greek learning came in 1453, when on that fateful 29th of May Mahomet the conqueror burst through the gate of St. Romanus over the corpse of the valiant Constantine, and the Greek Empire ceased to exist after a thousand years of mingled glory and shame. Christendom has rightly deplored the last act of that tragedy when the Crescent supplanted the Cross at Constantinople. But the historian records with satisfaction that out of the evil thus perpetrated untold good came to Europe. With the final overthrow of the Eastern empire the treasures of Greek learning were released, and became quickly disseminated throughout Europe. The Greek scholars fled and took up their abode in Italy. Ships coming from the Golden Horn carried the priceless manuscripts which the Turk, caring nothing for the learning of the infidel, had spurned. Italy received with kindness the scholar-refugee and his literature. The vanguard of those who were destined to restore the Greek spirit to Europe had already settled in that country, and soon the Italian universities were busily engaged in imbuing themselves with the new influence. Thither flocked students from every part of Europe, and among them one revered by this College, Thomas Linacre, our great founder. Scholasticism was swallowed up in the advancing tide of Humanism; learning revived, and with it medicine and science began to be studied again in a manner calculated to produce solid and lasting results.

Now if acquaintance with the condition of medical knowledge previous to the revival of learning be desired no better book can be consulted than the *History of Physick* written at the beginning of the eighteenth century by that admirable and learned Fellow of this College, Dr. John Freind. This work was composed in 1723, while Freind was lodged in the Tower under political suspicion, and does the greatest credit to his scholarship and vast knowledge of the medical writings of the ancients. In point of style the work is excellent, and although the first book to appear in this country dealing with the history of medicine, successive historians have been able to add little to our stock of knowledge of the subject. Freind's lively wit and the mastery of an easy style enabled him to present the history of medicine in a garb most acceptable to the reader. In a delightful vein of sarcastic pleasantry, worthy of his friend Swift in his happiest mood, he deals with the pretensions of many so-called medical authorities during the Middle Ages. From a study, then, of the pages of this Father of the History of Medicine in this country a clear view of the state of medical knowledge in the Middle Ages may be obtained, and Freind is fully worthy of being commemorated this day as a benefactor of the College, as a man of letters, and as a physician.

The condition of medical and scientific knowledge in the Middle Ages, as described by Freind, may be summed up in a few words. With the intellectual stagnation existing at that period in every branch of thought, progress could not be expected. Whatever mental energy was expended had for its aim the foundation of immutable principles that time and the ingenuity of the intellect could never alter. The learned might explain, but they might not criticize. Whatever had received the sanction of authority became a fixed belief, and steps were taken to protect it from doubt and destructive criticism. In fact, the ultimate end of all intellectual effort during the period of the Middle Ages was teleological, and so long as that spirit remained dominant no real advance in any branch of learning could be possible. All the knowledge that the Middle Ages possessed concerning medicine and science came from the Greeks, and even this was presented in an incomplete and inexact form. Through translation and repeated copying numberless errors had found their way into the classics, and it became a matter of no little difficulty to correct and purge them. How manifold were these errors will be understood when it is remembered that Hermolaus Barbarus restored no less than two thousand corrupt passages in the thirty-six books of Pliny. The Middle Ages, previous to the revival of learning, represented, therefore, a stationary period in which, as Whewell has justly observed, learned men could be produced, but not discoverers.

But with the revival of learning in the fifteenth century came a far-reaching change in the attitude of students towards the study of medicine and science. Hitherto they had been content to accept what had been sanctioned by authority. They were not competent to determine whether the texts they read were exact translations from the original or merely corrupt compilations by ignorant scribes. The recovery of the Greek tongue transferred medicine from men of this class to physicians deeply versed in classical knowledge, who combined the study of humanism with that of medicine and science. Their examination of the Greek texts enabled them to detect the violence that the translators had committed in their corrupt translations. These humanist or scholar-physicians loomed large in the first half of the sixteenth century, and it has been the desire of some to treat their work with scant respect. But let us remember that

without their aid it would have been impossible to revert to the original Greek texts, that they threw off the yoke of the Arabian translations and enabled students to go with safety to the Greek fountain-head of medical knowledge.

Many of the scholar-physicians had acquired their knowledge of Greek in Italy, where the study of that language had reached a high degree of perfection. But of all those who made a pilgrimage to the shrines of the new learning in Italy the great figure of Thomas Linacre, the founder of this College, and the best example of the scholar-physician, possesses for us an absorbing interest. It will not be necessary for me to recount the main facts of his career, and indeed to do so in this College, his ancestral home, would be an act of impertinence. But I may with propriety comment upon certain features of his life and activities in order to assess the enormous debt this country owes to his learning, and to his efforts to improve the condition of medicine. Already proficient in the Greek language, through the influence of his friend and master, Selling, he travelled to Italy for the purpose of placing himself under the best teachers of the classics. Arrived there, he sat at the feet of Politian, to whom he owed his exquisite taste in Latin composition, and of Chalcondylas, who refined his knowledge of Greek. But while engaged in the pursuits of classical knowledge his thoughts became directed to the study of medicine, and after a residence of a few months at Padua he graduated in medicine at that university. Linacre had spent two years only in Italy, and some have pointed to this short preparation as evidence that the exercises for the degree were merely perfunctory. But in those days a knowledge of medicine as taught by the ancient writers was all that was demanded for the degree of doctor, and Linacre, we may be sure, while studying Greek and Latin, had become fully conversant with the medical texts, so that he could perform with ease the exercises required.

The writings of Galen appear to have attracted him most, for in after-years he confined his attention to that author, and devoted his unrivalled scholarship to the purpose of providing correct and elegant Latin translations of his more important works. The success of these translations was great, and largely owing to their popularity the authority of Galen became paramount during the early part of the sixteenth century. Indeed, so sacred was the teaching of Galen during part of the century that John Geynes, a Fellow of this College, was fined and threatened with expulsion for daring to impugn his authority. These translations of Linacre showed a distinct advance when compared with similar writings during the Middle Ages. They were not commentatorial, but merely good and exact translations from the original Greek, and enabled students of medicine, probably for the first time, to read Galen untrammelled by the useless and prolix commentaries of inexact and mediocre translators. After a perusal of these correct translations came the inevitable scientific sequence of doubt and inquiry.

But to Linacre belongs a glory far in excess of any he obtained as a scholarly translator. Even the solid basis on which his fame rests as the first to revive letters in this country must yield to the position he occupies in our hearts as the great founder of this College—the first institution devoted to the conservation and advance of medicine in this realm. He had visited Italy, then the only place where the influence of the revival of learning was felt. In the various universities where he had pursued his studies, and where he had consorted with men of learning, he was impressed with the manner in which facilities were afforded for the prosecution of the quest for knowledge in all its branches. Padua, then nearing the zenith of its influence, must have had a profound effect upon his mind. No doubt he compared these busy centres of learning with Oxford and Cambridge, still slumbering in the heavy and uninspiring atmosphere of sterile scholasticism. In Italy he felt intellectual life vibrating, and saw men striving to climb the heights unfolded by the revival of learning. In England he remembered only the droning of the commentators, the unprofitable exercises, and the thunders of the nominalists and realists. While preparing for his degree in medicine at Padua he had seen the attention paid to that subject. The care bestowed upon the study of medicine by the universities and the State in Italy sank deep in his mind, and when in after-years he resolved to improve the condition of physic in his own country it was after the Italian model that he shaped his College of Physicians. England had her universities devoted to learning, and now, through the inspiration of Thomas Linacre, she was to have her College of Physicians specially designed to guard the interests of physicians and to promote the study of medicine.

THE SCHOLAR-PHYSICIANS.

To Linacre, the scholar-physician, then, we owe it, and to him alone, that England soon after the revival of learning possessed a College endowed with adequate powers to guide and direct medical progress. The influence of that corporation upon the medical destinies in this country has been great. Throughout the course of centuries the College has retained the characteristics, indelibly impressed by the founder, of high purpose and catholicity of aim. For these reasons we of this College honour this day Thomas Linacre as the founder of our College of Physicians.

But the influence of Linacre bore fruit in other directions. With his friends Grocyn and William Latimer he was responsible for the introduction of the revival of letters in this country. Without that revival the scientific achievements of the seventeenth century could scarcely have been accomplished. Finally, towards the end of his life he designed a scheme for the promotion of the study of medicine in the sister universities, and although the practical application of that scheme fell far short of the intention of the founder, the credit for the idea belongs to Linacre. Indeed, it would appear that in these latter days the desire of Linacre to improve the facilities for the study of medicine in the universities is at last meeting with its reward. At Oxford the funds of the Linacre lectureships are now devoted to the teaching of physiology, while at Cambridge you, Sir, as the occupant of Linacre's chair, have been worthily entrusted with the pious duty of promoting the aims so earnestly desired by the founder.

Less than half a century had elapsed since the death of Linacre before the destinies of the College were placed in the able hands of another scholar-physician, John Caius, a worthy successor of the founder. But for his energy and perseverance it is probable that the influence of the College as a factor in medical progress would have been confined within narrow limits. Caius, like Linacre, was a scholar. He had trodden the same path in his quest for learning in Italy. Taking the founder as his model, he spent his life in promoting the cause of learning and medicine. His benefactions were conspicuous, and whether he was designing the regulations of the College which bears his name, or ruling over this institution as its President, his aim was ever the advancement of knowledge. Although a true type of the scholar-physician, he was the first in this country to write a treatise on clinical medicine, and his short work on the sweating sickness is the precursor of the work of Sydenham and Heberden. Natural history also claimed his attention, and with Edward Wotton, a Fellow of this College, he shares the honour of being one of the first to write on that subject in this country. Of the thoroughness of his work for the promotion of the interests of this College we have abundant evidence in the first volume of the *Annals*, written in his own hand. To the most minute detail he ordered everything, from the compilation of the first book of Statutes to the provision of the silver caduceus still in use, and his pious act in rescuing from neglect the tomb of the founder. In common with Linacre, Caius was strongly attracted to the writings of Galen, and spent much of his time when in Italy in making a careful examination of the manuscripts of that author. But his scholarship, although profound, was not cast in such a rigid mould as that of Linacre, and there is evidence that his mind was undergoing the emancipation inevitable with the ever-widening scope of learning taking place in his day. When in Italy he lodged with Vesalius, and from this association it is probable that he acquired his taste or anatomy. In after-years, when he refounded Gonville Hall, the study of anatomy received full recognition in the regulations of the College. He also lectured on anatomy before the Barber-Surgeons, and may thus be regarded as the founder of the study of anatomy in this country. For this, and his unselfish devotion to the interests of the College, we record our thanks this day.

Caius may be regarded as the last conspicuous example of the scholar-physicians, and it will not be out of place to attempt an estimate of the work they accomplished in the cause of medical progress. These men were deeply versed in all the learning of the age in which they lived, and their knowledge of medicine came to them in the ordinary course

of their studies. They were scholars first, and sometimes their interest in medicine was inconsiderable. This attitude will, however, occasion no surprise when it is remembered that the knowledge of medicine in those days was stationary, and did not, therefore, oblige the student to keep himself informed concerning its progress. So long as he knew the ancient medical authors, and the commentaries thereon, his knowledge could never be out of date. But when the revival of learning began to influence Europe the stationary period of medical and scientific knowledge soon showed signs of passing away, and it became no longer possible for the scholar to keep up with the advances that were being made. For the revival of learning had let loose upon the world experimental science, and scholarship alone was incompetent to deal with the problems that arose. For these reasons, therefore, soon after the middle of the sixteenth century, science, including medicine, became divorced from scholarship, and a new race of men arose who dared defy written authority, and who inquired only of Nature for the revelation of her secrets. In this way was prepared the road for the great scientific advances which took place in the seventeenth century, one of which was the appearance of Harvey's great work.

The scholar-physicians, then, of whom Linacre and Caius were such shining examples, in spite of their limitations, occupied no inglorious position in the history of medical and scientific advance. They effectually bridged the period between the end of the influence of the Middle Ages and the beginning of sound scientific methods. Their knowledge, it is true, was sought in the study, but their scholarship was invaluable to the medical age in which they flourished. They restored the pure texts of the medical classics, and thus enabled those engaged in the study of medicine and science to begin anew their investigations. The scholar-physicians were also deeply imbued with the higher aims of humanism. From this spirit sprang their desire to benefit mankind, which took definite shape in the foundation in this country of institutions designed to promote the study of medicine and science. In the early years of this College the Roll contained the names of many whose attainments in scholarship far outweighed their ability as physicians. Besides Linacre and Caius may be mentioned John Clement, Edward Wotton, and Richard Bartlot. These men created that spirit of wide culture in the College which remains to the present day. To the scholar-physicians, therefore, we render thanks, and especially must we hold in remembrance the work of Linacre and Caius in the foundation of this College and of Caius College at Cambridge.

Progress in the Sciences.

Attention must now be devoted to the history of the progress made in the sciences, after the ground had been prepared by the revival of learning. Soon after the beginning of the sixteenth century a reconsideration was undertaken of the data on which the sciences were based, and produced a change in the conceptions hitherto regarded as fixed. The first movement in the direction of progress came from the domain of astronomy, when Copernicus, discarding the Greek system, adopted the heliocentric theory. But his epoch-making work did not see light until 1543, although completed many years before, and its full fruition was postponed until Kepler formulated the laws governing the eclipses towards the end of the century. Botany also began to advance. This subject had remained in a stationary condition, and had been satisfied by the compilation of herbals in which the teaching of the ancients was alone regarded. The first to break away from ancient tradition was Leonicenus, who impugned the authority of Pliny; and he was followed by Fuchs, Gesner, and Caesalpinus, all of whom drew their facts from a study of nature rather than from books previously compiled.

Towards the end of the sixteenth century the science of physics was recast and placed upon a sound basis. Stevinus had established the principles of the inclined plane, William Gilbert, a distinguished president of this College, had disclosed the properties of the magnet, and Kepler had supplied the deficiencies of Copernicus, thus supplanting for ever the ancient astronomical system of Ptolemy. The laws of moving bodies were soon to be formulated by Galileo, and physics freed from its errors was in a position to lend its aid to the other sciences. With this progress taking place in physical science Aristotle became suspect, for certain of his ideas regarding physics failed to stand the test of modern criticism. His metaphysics had been used for centuries in support of the doctrines of the Church, and his works concerning the biological sciences had long been the textbooks of the schools. But when the physics of Aristotle had been called into question, chiefly by Galileo, it was unfairly concluded that other parts of his writings were open to criticism. Without justification, therefore, those works shared the fate of the physics, and for the greater part of the seventeenth century his authority suffered an eclipse which did not pass away until the eighteenth century.

Progress can be seen in whatever science is surveyed in the sixteenth century, for the age had arrived when men began to doubt. When this state of healthy scepticism took possession of the mind, inquiry, which must inevitably lead to the increase of knowledge, was the only logical course that could be pursued.

While these changes were taking place in scientific knowledge, a wider and more profound change was shaping itself in the realm of thought. For, towards the end of the century, Bacon was striving to exorcise from the minds of men the four idols, and was building up that system founded on inductive methods which he was persuaded would guide the intellect to just conclusions. The great philosopher, spurning the useless methods which produced no increase of knowledge, fashioned his system with the single aim of gathering the fruit of intellectual effort for the benefit of mankind. Although we can point to no signal achievement as the result of that system, yet it marks the definite severance from the ancient, and the beginning of modern thought.

The Objective Study of Anatomy.

But of all the sciences which showed a tendency to advance in the sixteenth century, that of anatomy claims our attention in an especial manner, for through the advance of that science lay the road to Harvey's great discovery. Anatomy, like the other sciences, had remained dormant until 1543, when a book appeared which exercised the most profound influence upon the subject. I refer, of course, to the *De Humani Corporis Fabrica* of Vesalius, a book from the appearance of which we may date the modern science of anatomy. Vesalius had been brought up to regard the teaching of Galen as beyond the sphere of criticism. But he dissected bodies himself. He used his eyes and found that what he saw was plainly at variance with the account given by Galen and sanctioned by immemorial authority. His mind was torn by conflicting emotions. In his day it required no little courage to impugn so sacred an authority, but eventually he was able to emancipate himself from the old dogmas. He described the human frame as he knew it from his dissections, and when that had been accomplished the days of the authority of Galen were numbered.

The Beginnings of Physiology.

After the publication of this system of anatomy by Vesalius the anatomists became uneasy in their minds regarding the teaching of the great Galen, but so firmly fixed was his authority that they hardly dared to do more than utter a half-articulate murmur of dissent. For the majority Galen had furnished for all time the true explanation of the manner in which the blood circulated and performed its supreme function of maintaining life, and they shrank from giving up that explanation sanctioned by time and authority. But their eyes told them only too plainly that much of that explanation was mere supposition and lacking in scientific proof.

What was this theory of the circulation of the blood, and its consequent rôle in the nutrition of the body, as advanced by Galen? At the risk of repetition it must be stated in order to appreciate the great induction of Harvey. Galen's explanation, which I summarize from an admirable description recently given by Dr. Charles Singer, was as follows: The food taken into the body became converted into chyle in the intestines, and was carried by the portal vein to the liver, where it was changed into blood, becoming there endowed with the essence of all living things, called "natural spirits." The blood thus manufactured and endowed by the liver was then carried by the hepatic vein and the vena cava to the right side of the heart. Here it was purged of its impurities, which found egress by means of the pulmonary artery and lungs to the external air. Then the venous blood thus purified ebbed to and fro in the veins and carried on the functions of nutrition. But a small portion of this venous

blood filtered through the septum of the heart by means of invisible pores into the left ventricle, where it came into contact with the external air which had reached that chamber by way of the trachea, the lungs, and the pulmonary veins. By this contact with the air and the innate heat of the heart the blood became endowed with a higher form of spirit, termed the "vital spirit," and became arterial blood, which, ebbing and flowing in the arteries in the same way as the venous or crude blood, carried on its higher functions of nutrition. But the arterial blood which flowed to the brain became endowed there with the highest form of essence, known as the "animal spirits," and was distributed to the body by means of the nerves, which were held to be hollow canals.

The main supports, therefore, of the hypothesis of Galen were the liver as the source of the veins and the blood; the communication between the right and left sides of the heart through minute and invisible pores in the septum; the presence of three vitalizing essences; and the absence of any circular movement of the blood propelled by the heart. This system, partly based on anatomy and partly on hypothesis, was accepted by the world for more than a thousand years. It postponed and fettered any advance in physiology, and on biological sciences generally imposed restrictions similar in effect to the theory of phlogiston on chemistry.

After Vesalius had broken the ground, anatomists who had sat at his feet carried forward his work until by degrees most of the supports of the Galenic hypothesis were weakened. Servetus, Columbus, Caesalpinus, and Fabricius contributed to the work of destroying the Galenic theory of circulation, but although they gave indications of the coming light they all lacked the power to draw the all-supreme conclusion. The complete demonstration of the manner in which the blood circulated and nourished the tissues required the work of a genius, and that genius was William Harvey.

In one short work, in size little more than a tract, he proved by a direct appeal to the phenomena of nature that the teaching of Galen concerning the circulation was wrong. He laid the sure foundation upon which almost the whole of our modern conceptions regarding physiology are based. In the history of scientific thought he takes his place beside Copernicus, Galileo, Newton, Lavoisier, and Darwin. By the intuition of genius he was able to sweep away false ideas and systems grown sacred by lapse of time and in their place to implant sound views acquired on the journey from scepticism to inquiry. Then the book in which Harvey expounded his theory is a model of close and accurate reasoning. Step by step each position is made secure before a new proposition is demonstrated. The whole is then welded together and the inference drawn. Harvey once remarked with a sneer that Lord Bacon wrote philosophy like a Lord Chancellor, and yet the *De Motu Cordis* is perhaps the most perfect example of the inductive method as enunciated by Lord Bacon's vast intellect. Indeed, it is hard to believe that Harvey, who knew Bacon well, was not influenced to some extent by that capacious mind.

Harvey, then, established the primacy of the heart as a muscular organ propelling the blood in the action of circulation, and explained how the blood found its way from the right to the left side of the heart. When these important facts were made secure the whole of the physiological doctrine of Galen fell to the ground, and the science of physiology was left free to pursue its logical course. It would be wrong, however, to assume that these revolutionary views found instant acceptance, for it could not be expected that a system that had endured for centuries would cease to exist without a struggle. For a time efforts were made to seek a compromise between the old and the new physiology. Even Harvey could not altogether emancipate himself from the glamour of the great name of Galen, and throughout his work, which was directed to the subversion of the ancient authority, he always treats him with respect. But eventually Harvey's doctrine became supreme, and by the time that he was carried to his grave in 1657 all but an insignificant minority accepted his views without reservation.

It has sometimes been assumed that the advent of Harvey's book at once displaced the whole of the system of Galen, including his medical teaching. This conception of the effect of Harvey's work is far from the truth, for, as a matter of fact, it had but slight influence upon the practice of medicine during the seventeenth century. The reason for this will not be far to seek when it is remembered that modern physiology began with Harvey, and had to make considerable advances before it could be of real assistance to medicine. A long and distinguished line of physiologists, ending with Haller towards the middle of the eighteenth century, had to accomplish their work before physiology was able to come to the aid of practical medicine. The science of chemistry, also, was uncreated, and it was not until the end of the eighteenth century that it was in a position to lend its powerful aid in the study of medicine.

The impetus given to the investigation of the problems of physiological science by the appearance of Harvey's work was truly marvellous. Before his day scarcely any attempt had been made to approach the subject from a rational point of view. But after the advent of the *De Motu Cordis* the progress of physiology was triumphant and steady. When we think of the successful labours of Aselli, the discoverer of the lacteals; of Borelli, who brought to the subject the new physics established by Galileo; of Malpighi, the father of our knowledge of the glands and the tissues; of Glisson, who first enunciated the doctrine of "irritability"; of the work of Willis and Lower, we can only be amazed at the advance made in a short time largely through the inspiration of the work of William Harvey.

This College justly acclaimed Harvey as its brightest ornament and conferred upon him its most signal honours. His work supplied the incentive to several Fellows who, adopting his methods, contributed in no small degree to the enlargement of the boundaries of scientific knowledge. Among those who followed Harvey's precepts in the period immediately succeeding the appearance of his book I record the names of Francis Glisson, Thomas Wharton, Thomas Willis, Richard Lower, William Croone, Nehemiah Grew, Edward Tyson, and Clopton Havers, all of whom are worthy of mention on this day for the work they accomplished for science. But the teaching of Harvey left the majority cold, and did not exert any considerable influence upon their methods of investigating disease. Throughout the seventeenth century physicians were content to inquire regarding morbid phenomena in their studies, rather than at the bedside, until they were aroused from their speculations by the work of a man who stands pre-eminent in the annals of the College: I refer to Thomas Sydenham.

SYDENHAM.

Among the successors of Harvey the name of Sydenham finds a distinguished place. But, great as he was, by no exercise of the imagination can he be regarded as being influenced by Harvey's epoch-making work. Nor can he be properly admitted to the circle of men who fashioned the distinctive type of seventeenth-century thought. He stood alone in his glory. Knowledge gained from books he despised. The first-fruits of science garnered so marvellously in the beginning of the century were neglected by him, and even the new teaching of anatomy and physiology found him unresponsive. Indeed, except for a great admiration for the work of Bacon, we can hardly point to a single fact warranting us to link him with the spirit of the age in which he lived. He clung tenaciously to the humoral pathology and was uninfluenced by all the vast changes taking place around him. He was not a scholar; he had a contempt for science, and yet, in spite of these defects, he has left upon medicine in the seventeenth century an impression that will last as long as mankind requires medical aid.

Sydenham was essentially an innovator. He lived in the seventeenth century, but was not of it, and while others were painfully exploring the road of scientific advancement he, with the Father of Medicine as his only guide, fashioned his own path and for all time laid the foundation of clinical medicine. He was eminently fitted for the task of introducing new methods with the stern and vigorous Puritan blood in his veins; a real rebel; a fearless iconoclast. He was the first of a long line of distinguished physicians who raised the art of clinical medicine to its highest pitch. This country has always been renowned for its great clinicians, and the position to which they have attained has been due almost entirely to the influence of the teaching of Sydenham. If it should be asked what effect that teaching had upon the art of medicine, the answer would be given correctly by pointing to the work of Heberden, Bright, Addison, and Watson. His method was scientifically sound, and while others were attempting to make signs and symptoms conform to theory and authority Sydenham went straight to the

bedside to gather facts and make observations. In this he was true to the inductive method: a collector of facts from which to induce general principles.

Puritanism in the seventeenth century produced its Cromwell, the embodiment of relentless energy and resistless brain force; its Milton, who showed to what heights of grandeur and majesty our own dear language can attain; and its Bunyan, who, all unlettered as he was, in the simplest and sweetest vernacular penned the finest allegory the world will ever read. To these great productions of Puritanism may we not with justice add the name of Thomas Sydenham, the great Puritan physician, who founded for all time and for all countries the art of clinical medicine.

Conclusion.

Neither time nor the scope of this oration permits us to pursue further the immediate and remote effects of Harvey's work. Enough, I trust, has been said to show the comparative dearth of scientific endeavour before the date of his work, and the stimulus scientific research received on account of its appearance. In common with all other results of genius, attempts have been made to claim for others the honour of priority in promulgating the central ideas contained in Harvey's book; but they avail nothing. Copernicus was not the originator of the heliocentric theory, and Darwin gave to William Charles Wells the credit of having enunciated first the theory of evolution. But their glory is not diminished by these admissions, for ideas without irresistible proof are often the common property of the human intellect.

I do not propose to detain you with a consideration of the work of Harvey concerning generation, for that subject was dealt with last year in a most able and exhaustive manner by Dr. Herbert Spencer, who brought to his oration the accumulated study and experience of an expert. But there remains Harvey's character as a man, and that is a subject upon which it is good for us to dwell. He lived in an age when men, on account of the state of political and religious feeling, were unusually prone to make enemies, but all that has come down to us regarding his character does infinite credit to the rectitude of his conduct in all the relations of private life. He seemed to be uninfluenced by the strife going on around him, and although attached to the Royalist cause by service and interest he was able to survey the shipwreck of that cause with fortitude and equanimity. His lofty mental stature rendered it impossible for him to stoop to vulgar polemical disputes, and whenever he was obliged to enter the lists in vindication of his scientific views it was always with dignity and forbearance. The real character of Harvey is admirably portrayed by Sir George Ent in his well-known account of his visit to him in his old age, when he found him forgetting the lacerations of the mind in the pure joy of searching and studying out the secrets of nature by experiment. Kings might lose their heads, and old orders might be subverted for a season, but Harvey's gaze was fixed upon the beacon of eternal truth. His devotion to this College was an outstanding feature of his life. As its most distinguished Fellow he had conferred upon it the highest honour, and in a practical manner he exhibited his deep regard by his benefactions. His solicitude for the welfare of this noble library found expression in the erection of a building, a gift of books, and a bequest of money. A few years after his death the whole of the library, with the exception of 140 volumes, was destroyed in the Great Fire, but that disaster proved to be our richest gain, for it produced the great bequest of the Marquess of Dorchester to which we owe the rarity and value of our library. No doubt the liberality of the marquess was in part actuated by the injunction of Harvey to emulate those who had benefited the College, and in our day the bequest of Dr. Lloyd Roberts, second only to that of Lord Dorchester, was surely due to the same spirit. This library, raised up and maintained by the spirit of Harvey, is the noblest monument we possess of that wide intellectual culture with which this College has always been prominently identified.

Harvey, then, in his life and work is the shining example to this College, and the injunctions he has laid upon us in his bequest in 1656 may be followed safely by all who desire its welfare. In these modern days, when we stand at the threshold of momentous changes, we may still look for guidance and inspiration from those injunctions, for they aptly express our duty as physicians, as men, and as Fellows of this ancient and honourable College.

An Address
on
THE PAST, PRESENT, AND FUTURE OF MIDWIFERY.*

by

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My first words in addressing you as Professor of Midwifery and Diseases of Women in the University of Edinburgh must be to express my appreciation of the great honour which has been conferred upon me by my election to the chair. That honour is the greater by reason of the fame of so many of its former occupants, among whom were the leaders of their day in this particular branch of medicine. In the past Edinburgh has always been in the very van of obstetrical and gynaecological advance. To maintain that position among the medical schools of the old and new worlds is no easy thing. My feeling of pride is, therefore, tempered by a very deep appreciation of the responsibility of my position, and by a realization of the nature of the task which lies before me.

I take up that task with a determination not to spare myself in its accomplishment and with a hope that if I fail it may at least be possible to say of me that I tried. To help me I have the inspiration and training which I received from my former chiefs in Edinburgh, the late Sir Alexander Simpson and Dr. A. H. Freeland Barbour, to whom I owe so much. I have also the experience gained during the past ten years as professor of obstetrics and gynaecology in the University of Toronto. When I was called there, ten years ago, it was to take from Edinburgh to Canada the methods of teaching and training which had made the Edinburgh school famous throughout the world. While in Canada I never forgot that and the responsibility to my Alma Mater which it involved. And now, in returning to Edinburgh, I feel that I have that same responsibility to the University of Toronto and to Canada in bringing here all the best that I have learned in that large and progressive medical school and in that great and growing dominion.

In these days of rapid advance in all the medical sciences we are sometimes apt to forget what our predecessors did and how much we owe to them for our present-day position. It does good, therefore, every now and then, to look back a little and inform ourselves of their work, and of the difficulties they had to overcome. When we do so we are invariably surprised at the extent of their knowledge and their great foresight.

In this respect there is no more fascinating study than that of the history of the chair of midwifery in this university. I shall only touch upon a few of the more interesting facts, culled largely from the very full account compiled by the late Sir Alexander Simpson, as the result of exhaustive research into old records and manuscripts, and delivered as an inaugural address to his class in the year of the tercentenary of the university.

From these researches there appears to be no doubt that to Edinburgh belongs the distinction of having the first chair of midwifery. It was instituted on February 9th, 1726. At that time no medical school in Britain or on the Continent had such a professorship. Strasbourg instituted a professorship of midwifery in 1728, but apparently no appointment was made till some years later, while a century elapsed before Paris followed. The Edinburgh appointment was made by the town council, the patrons of the university. Their minute, as quoted by Simpson, is, in part, as follows:

"The Council having considered the petition of Mr. Joseph Gibson, chirurgeon of Edinburgh, with a declaration under the hands of four doctors of medicine, setting forth the usefulness and necessity of instituting a profession of midwifery, as also an extract of an act of the Incorporation of Chirurgeons, showing their approbation of the qualifications and capacity of Joseph Gibson, one of their number, for teaching the said art and science; they are fully convinced that it would be of great use and advantage to institute this profession, and being well satisfied with the ability and capacity of the said Joseph Gibson to discharge that office, they were of the opinion that the Council should nominate and appoint him to be Professor of Midwifery in this City and privileges, with power to him to profess and teach the said art, in as large an extent as it is taught in any city or place where this profession is already instituted; and that he should be vested with

*An inaugural address to the students of the Faculty of Medicine, University of Edinburgh.

the same privileges and immunities that are known to appertain to a Professor of Midwifery in any other well regulated city or place; but that it should be expressly provided that he should have no fee or salary from this city out of its patrimony or revenue, on account of his said profession. . . ."

The minute goes on to relate the dangers to which women were exposed as the result of the practice of untrained midwives and lays down regulations for licensing them. Apparently the chief duty of the new professor was to teach midwives, in whose hands practically all obstetric practice was. His title was "City professor" or "Town's professor," and he was not admitted as a member of the Medical Faculty of the university. His successor, Robert Smith, was appointed in 1739, and his commission constituted him a member of the Senatus Academicus, although he was not admitted as a member of the Medical Faculty.

Smith was succeeded in 1756 by Dr. Thomas Young, who must have done a great deal for the teaching and training of midwives, for his successor, Professor Hamilton, stated that as a result of his work there was "scarcely a parish of Scotland the midwife of which has not been regularly taught." Dr. Young was the first to give regular courses of instruction to medical students, and he was the first to have a ward set apart for their clinical instruction. Again quoting from Simpson:

"A ward in the attic story of the hospital" (the Royal Infirmary) "by permission of the managers, but at Dr. Young's expense, was fitted up for four lying-in women, or as many more as Dr. Young could accommodate, each exceeding the number of four paying sixpence per day to the house."

The work of these early professors must indeed have been a labour of love. The town council made it perfectly clear that they would receive no salary, and the Royal Infirmary granted Dr. Young the privilege of fitting up his ward at his own expense.

The chief facts of local and general interest in the career of Young's successor, Dr. Alexander Hamilton, are his admission to the Royal College of Physicians, which up to that time had not recognized those practising midwifery, and his establishment of the first maternity or lying-in hospital in 1791, under the patronage of the town council.

In 1800 he was succeeded by his son, Dr. James Hamilton, a man of great ability and a keen controversialist. His lectures became "so popular that there were few students who did not attend them" (Christison). In 1815 he applied to the Medical Faculty to have his lectures made compulsory.

"The Faculty declined his request, being guided by its three senior members, who had long been on no friendly footing with the applicant. Therefore, in 1824, he applied to the council to exercise their authority by passing an Act compelling the Senatus to add his class to the curriculum for graduation. By this procedure he played into the hands of the town council, whose object was to find a ground for raising an action of declarator of right to govern the university as of old" (Christison).

This action of Hamilton's was keenly resented by the Senatus Academicus, but in 1825, along with other changes in the medical curriculum, it added midwifery as a compulsory subject. The regulation was, however, not enforced till 1830.

Hamilton died in 1839, and was succeeded in 1840 by a young man of 29 years. The contest for the chair was keen, only a single vote dividing the successful candidate from his nearest rival. The man elected was James Young Simpson. Nothing that I can say can add anything new to the many appreciations which have been written of that man of genius. His name will live for ever as the discoverer of anaesthetic properties of chloroform and as the first to apply general anaesthesia for the relief of the pains of labour.

But Simpson did a great deal more than that for midwifery and for gynaecology. He was among the first to recognize the nature of child-bed fever, and two years before Lister gave to the world the principles of antiseptic surgery he wrote a paper pointing out the identity in the clinical course and autopsy findings of surgical and puerperal fever, and ascribed both to contagion carried by the hands or person of the operator. He was a keen clinical observer, and always found time to put his observations on record. He invented and perfected many obstetrical instruments, and by his use of the uterine sound and the sponge tent gave a great impetus to gynaecology. Academies and societies in every part of the civilized world bestowed on him their honorary titles, and he brought great renown to Edinburgh.

Throughout the thirty years he occupied the chair he was an enthusiastic and inspiring teacher; under him the status of midwifery in the curriculum was firmly established, and students of this university went forth with a better training than could be obtained in any other medical school. That at once made itself apparent in medical practice in Scotland, for midwifery since then has occupied a more important place in general practice in this than in any other country. This is one of the results of all the hard-fought fights of our predecessors, and one which we must strive never to lose; there is a very real danger of its being lost, as we shall see later.

In 1870, on the death of Sir James Young Simpson, his nephew Alexander Russell Simpson was elected to the chair, which he occupied till 1905, when, after thirty-five years' service, he resigned, because, as he said in his valedictory address, "there is always risk that the ageing incumbent of a chair may become an incubus upon it. I could not bear to dree that weird." In his early years he had studied extensively on the Continent, and was always a good linguist. He travelled much, and knew personally all the best of the workers in his subject in Europe. In this way, and through his extensive writing, he kept the Edinburgh school of midwifery in the forefront. He was a skilled operator, and in the early days of abdominal gynaecological surgery did much pioneer work. His lectures were always up to date, as he was constantly adding to them from his personal experience and from his wide reading. His delivery was not impressive, but the material was of solid worth, and notes faithfully taken proved better and fuller than any textbook of the day. My own notes of these lectures are among my most valued possessions. He did much for the clinical teaching of midwifery, and inaugurated definite clinical instruction in gynaecology.

As a chief in the hospital he was exacting, but when the morning's work was over and his resident accompanied him to the gate of the infirmary he became the kindly adviser and friend, betraying that keen sense of humour which under other circumstances did not show itself. He received the honour of knighthood in 1906, a fitting recognition of his work for humanity. His untimely death in 1916 was mourned not only throughout Scotland but in distant lands where his Christian charity and sympathy had won for him a place in the hearts of many peoples.

On Simpson's resignation it was deemed advisable to divide the duties of the chair, and Sir J. Halliday Croom was appointed professor of midwifery and Dr. A. H. Freeland Barbour lecturer in gynaecology. This division was in conformity with what had been done in many other medical schools, especially on the continent of North America. It had been felt that gynaecology had made such rapid strides that it required the undivided energy of one man to teach it in the medical school and practise it in the hospital. In many places this separation resulted in a complete divorce of gynaecology from obstetrics, and the former came largely within the domain of the general surgeon. As a result, there were gynaecologists who had had no extensive experience of obstetrics and obstetricians who had never practised gynaecology. Much that was common to both was lost sight of. The gynaecologist was apt to exaggerate the surgical side of his subject, to lose sight of the etiological factor in many conditions, and he had necessarily no control of what we shall speak of later as preventive gynaecology. On the other hand, the obstetrician who was not also a gynaecologist was deprived of opportunities of perfecting himself in operative technique, and to-day surgical intervention is more and more resorted to in obstetrics as being in many instances the safest and most conservative line to adopt in the interests of mother and child. These considerations, among others, have caused the pendulum to swing back, and now opinion, if not unanimous, is overwhelmingly in favour of the linking up of the two subjects again. So in 1912 I was called to the University of Toronto to reunite the chairs of obstetrics and gynaecology, which had long been separated, and now in 1922 I return to Edinburgh to teach and practise both subjects.

In Edinburgh, although the chair has been divided during the past seventeen years, there has been no divorce between the two subjects such as had taken place in many other schools. Sir J. Halliday Croom is equally distinguished as obstetrician and gynaecologist; Dr. Barbour has done lasting work in obstetrics; and there was the closest co-operation between them in the teaching and training of the students.

It is difficult in their presence to say all that I should like to in appreciation of their services to the university and the Edinburgh school. Sir Halliday Croom, long before he was called to the chair, had made a wide reputation by his

writings. As a teacher in the extra-mural school he gained such popularity that for a generation medical students in Edinburgh did not consider their education complete unless they had attended his summer course. His enthusiasm for his subject was unbounded, and he presented it to his pupils in such a clear and dramatic way that they could not fail to be interested and impressed. This summer our university conferred upon him the honorary degree of Doctor of Laws in recognition of his worth. We all join in wishing him long years in which to enjoy his retirement.

Dr. Barbour's name has an abiding place in obstetrical literature by reason of his work on the anatomy of labour, a work accomplished while he was still a young man. Along with the late Dr. D. Berry Hart he published a textbook of gynaecology which was translated into many languages, went through many editions, and remains a classic. His contributions to periodical literature have been many, and all are distinguished by originality. The men he has trained occupy distinguished positions in the medical schools at home and on the other side of the Atlantic, and only those of us who have had the privilege of working under him know how much we owe directly to him.

The history of the chair of midwifery in Edinburgh is practically an epitome of the whole history of obstetrics during the past two hundred years. We see first of all the effort to train midwives in the appointment of the first professor in 1726; the gradual extension of that teaching and training to medical students by the provision of voluntary classes for them, and the setting apart of wards for their clinical instruction by Professor Young; the recognition of the status of the obstetrician by the election of Professor Alexander Hamilton as a Fellow of the Royal College of Physicians; the admission of Professor James Hamilton to the Medical Faculty of the university, and the institution of compulsory training and examination of students proceeding to a medical degree; the introduction of anaesthesia in midwifery practice by Sir James Simpson; the recognition by him of the nature of puerperal fever, the combating of which was begun by his successor, A. R. Simpson, as the result of the discoveries of Lister; the invention of obstetrical instruments and the perfecting of operative technique by Sir James Simpson and his successors. And all along obstetrics and gynaecology have benefited by discoveries and developments in other departments of medical science—in anatomy, in physiology, in pathology and bacteriology, and in physics. Thanks to the discoveries of Simpson and Lister, technique has been so perfected as to render the performance of even the most severe obstetrical operations almost devoid of risk to life, and the whole field of operative gynaecology has developed.

Yet there remains a high puerperal mortality, both maternal and foetal. Puerperal fever alone takes a toll of from one to three mothers per thousand births. While the mortality from this disease has been enormously reduced in our hospitals, a corresponding reduction has not been achieved in private practice. The conclusion must be that antiseptic and aseptic principles are not followed with the same careful attention in the patient's home as in the hospital. There can scarcely be any doubt that if the principles of antiseptic and aseptic surgery were applied as rigidly in private as in hospital obstetric practice the mortality from puerperal sepsis would practically disappear. A great responsibility attaches to us as teachers, and to you as students and future practitioners, in this matter.

There is still also a huge maternal mortality from haemorrhages, from the toxaemias of pregnancy, and from complications arising during labour. The numbers of stillbirths—in Edinburgh in 1921 the number was as high as 47.8 per 1,000 births—and of prematurely terminated pregnancies mean a great loss of human life. Women in large numbers still suffer from disabilities, causing great suffering and loss of efficiency, as the result of injuries received in the process of childbirth, and neglected at that time.

With all that has been left us by our predecessors we cannot but think that we ought to be doing better. And, given the proper facilities, there is no question but that we *can* do better.

It has long been recognized that most of the complications arising in pregnancy, in labour, and after labour are preventable. If they are detected early they can be easily cured. In the case of most of them it is only when they have progressed, unrecognized and untreated, that they are dangerous. The progress which midwifery has made in the last twenty years, and is destined to make in the next twenty, is along the lines of preventive medicine. Preventive medicine as applied to obstetrics means ante-natal care during pregnancy, a minimum of interference during labour, the most rigid attention to asepsis, and the earliest possible carrying out of surgical interference when it is required.

Ante-natal care implies a thorough general examination of the patient as early in pregnancy as possible, and a special examination to make sure that she has the physical configuration necessary for a normal labour. It implies a careful watch on the patient at regular intervals throughout the pregnancy, and the immediate institution of appropriate treatment whenever the least departure from normal is detected.

Unfortunately it is difficult to get women to realize the importance of this careful watching, with the result that the doctor is often only called in when it is too late. The public has to be educated, and it is only through you that that education can be carried on, and you yourselves must first be taught.

We in Edinburgh must not lag behind in this work, for here in our medical school we have the man who foresaw all this present-day development many years ago. I refer to Dr. J. W. Ballantyne. Thirty years ago he was already greatly interested in and an authority on pathological conditions of the foetus. In 1901 he published a paper entitled "A plea for a pre-maternity hospital," and probably largely as a result of that paper a pre-maternity bed, known as the "Hamilton Bed," was endowed in the Royal Maternity Hospital. This was probably the first specific provision for ante-natal care of patients in any hospital. In 1915 Dr. Ballantyne established and has since conducted the ante-natal clinic attached to the hospital.

It is now universally recognized that a pre-maternity or ante-natal department is a most essential—I should say the most essential—part of a maternity hospital. In addition to the outdoor clinic there must be an adequate number of beds for women suffering from the various complications of pregnancy. A hospital which provides only for the women actually in labour, or already delivered, is falling far short of the best that it can do. In the obstetrical department of the Toronto General Hospital, of which I had charge, patients at all stages of pregnancy were admitted whenever their condition required it. Our ante-natal clinic was established there in 1913, and I have always felt that that was the most satisfactory part of my work.

The results of it were shown in the absolute prevention of eclampsia in the patients attending it, the early detection of pelvic deformities and their appropriate treatment, the early treatment, and in most cases cure, of the toxic vomiting of pregnancy, and in other ways too numerous to mention here. What we did in hospital we did also in private practice. We found no difficulty in getting our patients to come to see us early in pregnancy and at frequent intervals throughout. There is nothing that so lightens the burden of obstetric practice as this knowing of the patient beforehand, and there is nothing that is so comforting and reassuring to the patient, as she approaches her time of trial, as the assurance already given her that everything is proceeding perfectly normally.

The problem which faces us is to convince women generally of the necessity for ante-natal care. This, as I have said before, can only be done by the efforts and example of the whole body of medical practitioners. An isolated ante-natal clinic here and there will benefit the community which it serves, and public opinion will be educated to a certain extent, but it will not be until the consulting room of every practitioner is an ante-natal clinic for his district that the maximum of benefit will be obtained.

Along with ante-natal care must go post-natal care; in addition to ante-natal clinics we must have post-natal clinics. As the ante-natal clinic is the great means of prevention of obstetrical complications so the post-natal clinic is the means of prevention of many gynaecological conditions. Post-natal care is preventive gynaecology. No patient should leave a maternity hospital, or be discharged by us as a private patient, until we are satisfied that she has suffered no physical disability as the result of her labour. We can give no such guarantee at the end of ten days or two weeks—the usual duration of her stay in hospital. The ideal we must aim at—and we must always be aiming at ideals—is to get every patient to return for examination four weeks after childbirth. That examination should be made at the patient's home, in the doctor's consulting room, in the post-natal clinic of the maternity hospital, or in the child welfare centre, to every

one of which I should like to see an obstetrician attached for the purpose.

Here again it is a matter of educating the public. That it is worth while attempting is shown by the numbers of women who fill our gynaecological wards suffering from distressing conditions resulting from obstetrical injuries. Had they been recognized at the time they could easily have been rectified. Neglected as so many of them are, they necessitate severe and often hazardous operations.

These phases in the development of obstetrics come at a time when in this country a great part of midwifery practice is in danger of passing out of the hands of the medical profession into the hands of certified midwives, and it looks as if the great fight which our predecessors made to give to the women of Scotland the benefit of skilled attention from properly trained doctors were to be in vain. It must be remembered that England was much behind Scotland in this regard, and it almost looks as if we, as the result of legislation common to both countries passed in the last few years, were making a retrograde step. I grant that the training these midwives receive and the work which they do is good, but very often that training is received at the expense of that of the medical student, and the work of the midwives, especially in this all-important branch of ante-natal care, is limited by their having no general medical education.

If we in Britain are to keep abreast of what our cousins overseas are doing we must be prepared to give a much more thorough and intensive training in midwifery to our medical students. Such training cannot be given by making the attendance on a certain number of cases at the patient's home the chief feature. Clinical medicine and clinical surgery could never be taught in that way, and clinical obstetrics certainly cannot. Clinical medicine and clinical surgery cannot be taught by a few isolated clinics given at intervals throughout the session; neither can clinical obstetrics. What is necessary is intensive training. A certain part of the medical course must be set apart for clinical obstetrics and gynaecology, so that the student may concentrate upon them—live, as it were, in the atmosphere of them. During that time he ought to see all the work of the maternity hospital, the ante-natal clinic, the technique in the labour room, the care of the newly born child, the after-care of the mother, and the post-natal clinic.

I have already had experience of this system of intensive clinical training, which I inaugurated in Toronto eight years ago, and I can speak for its efficiency. The objection that the intensive system leads to too rapid presentation of the subject for its thorough assimilation is got over by a continuation of regular clinical lectures to the whole class, say once a week, throughout the last year of the medical course. That also was a feature of our teaching in Toronto.

With such a system it is possible to make the maximum use of all the clinical material available, which under other conditions is largely wasted so far as teaching is concerned. I hope that it may be possible, with the co-operation of my colleagues in my own and in the other clinical subjects, to inaugurate some such system here in the near future. It is simply a matter of arranging the time-table, for I know that they are in accord with me as regards the principle and had arrived at the same conclusions some time ago. The clinical material is here in Edinburgh if we can only use it, and I hope the time may not be far distant when it will be unnecessary for any of you to seek your clinical instruction in midwifery elsewhere.

The Royal Maternity Hospital and the university now work in the closest co-operation for the treatment of the patient and the training of the student. One of the urgent needs at the present time is the enlarging of that hospital and the adapting of it to modern requirements. Our hospital ought to be a place where the future practitioners of midwifery can see obstetrical work conducted in a manner as near the ideal as human effort can make it. It ought to be fitted up to provide all the things which I have mentioned—the ante-natal clinic, ante-natal beds, and with operating and labour rooms, where perfect aseptic midwifery can be carried out.

There is no more urgent need at the present time than funds for the carrying out of this extension and enlargement of the Royal Maternity Hospital of Edinburgh. Until we get it we cannot do all that we ought to for the training of our future practitioners. We cannot be anything but embarrassed when we have to show to our colleagues from distant lands, who visit this city and university so famous in the history of midwifery, a hospital which, though erected to the memory of Sir James Young Simpson, is many years behind the times.

I can conceive of no more fitting memorial to the fallen, none which would do more to compensate for the enormous loss of Scottish life in the late war, than the erection and equipment of a modern maternity hospital in Edinburgh.

Some Observations ON THE BACTERIOLOGY AND VACCINE TREATMENT OF CHRONIC BRONCHITIS:

BASED ON 300 CASES, WITH SPECIAL REFERENCE TO ASSOCIATED NASAL INFECTIONS.

BY

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THE description "chronic bronchitis" does not convey quite accurately the condition which forms the subject of this paper, for the cases with which it deals fall into three groups which differ both in their severity and in their response to treatment.

1. Those who suffer from frequently recurring attacks of bronchitis but have an interval of several weeks' freedom between each attack.
2. Those who suffer from bronchitis throughout the cold months and are liable to acute exacerbations, but are comparatively free from cough or expectoration during the warmer months.
3. Those who are never free from bronchitis, but are worse during the winter, and are liable to acute exacerbations at any time.

All are liable at some stage to develop asthma as a complication of the bronchitis.

Nearly all such patients find our climate a great handicap, and they would benefit by residence in a warmer and drier land, but very few can afford such a change even during the winter months. It is no secret that ordinary medicinal treatment is disappointing, both to such patients and to their doctors; as time goes on the bronchitis gets worse, and eventually changes in the bronchial tubes, lungs, and heart render the patients incurable invalids.

In the hope of bringing relief to such sufferers I wish to point out a way in which treatment by autogenous vaccines can be made more successful than it has been hitherto. It has generally been taken for granted that in such cases the autogenous vaccine must be prepared from the sputum, and sometimes this can be done with great success. But the practitioner knows little of the technical difficulties which the bacteriologist encounters in attempting to secure pure cultures of the causal organisms in a particular case. It may be easy enough to *see* the germs but impossible to grow what is seen. The particular sample of sputum may not contain the germs responsible for the catarrh, or may contain one variety only out of several which are responsible. In the most favourable circumstances it is rarely possible to obtain primary cultures in sufficient purity and quantity to make a vaccine, yet most of us feel that there is a virtue in primary cultures that is lacking in the succeeding generations of subcultures.

Then there are the difficulties we make for ourselves. The bacteriologist may not see the patient for whom he is working, he may not even hear the result of the treatment by the vaccines which he has prepared, and so has little opportunity of learning that some of the catarrhal germs are of fundamental importance and others of no account. Better to refuse to make a vaccine than to prepare one from germs which have no pathogenicity.

The impression still seems to obtain that a pot of sputum sent through the post, perhaps twenty-four hours old by the time it reaches the bacteriologist, is suitable material from which to prepare a vaccine. Such a sample is quite suitable to examine for tubercle bacilli, but certainly not for making cultures from which to prepare a vaccine. Such a sample is dangerous because it is fallacious—some of its germs have increased in numbers, other varieties have died. From such a specimen one may prepare an excellent vaccine, but it is largely a matter of chance.

When we also consider the possibilities of error in bacteriological technique, in deciding which germs to include in the

vaccine and which to omit, in adjusting the strength of the vaccine to the age, weight, and condition of the patient, we ought not to be surprised that a considerable proportion of cases so treated fail to make a successful response. Yet I become more and more convinced that if the vaccine is made of the right stuff no case of catarrhal bronchitis will fail to get some relief from it.

Whenever there is sputum to be had it should be carefully examined, but first it must be carefully collected. The patient should be instructed to wash his mouth and throat thoroughly with warm water on waking in the morning, and when next he coughs to expectorate into a short, wide, sterilized glass tube, which is then securely corked. If this first phlegm is partly greenish or yellow there is no need for the patient to collect any more; but if it is simply clear mucus he should try again, for a little purulent material is of great value. This sample of sputum should be sent at once to the laboratory by a messenger who will carry it next to his body or in his trousers pocket, and so keep it warm. Never more than three or four hours should elapse between the coughing of the phlegm and the preparation of cultures; this is so important that it is well worth while hiring a car to save time if a considerable distance separates the patient from the bacteriologist. Such a sample should be examined microscopically; particles should be chosen and cultured on blood-agar, and if the bacteriologist, guided by the microscopic appearance of the sputum and by the clinical symptoms, grows germs which seem adequate to account for the bronchitis, he can hopefully continue his preparation of a vaccine. If, on the other hand, he is not satisfied he should try again with another specimen of sputum. Every sputum is worth examining for tubercle bacilli, for occasionally they are found when least suspected.

Opinions may differ as to the relative importance of the germs found in the sputum in chronic bronchitis, and I will speak only from my own experience. In 130 cases I found germs in the sputum which seemed to me of sufficient importance to be incorporated in a vaccine, though not in all those cases was I content to rely on the sputum alone. The germs so used and the number of cases in which the various organisms were found were as follows:

Pneumococcus	53=40.0 per cent.
M. catarrhalis	42=32.5 ,,
B. influenzae	58=44.0 ,,
Friedländer's bacillus	10= 8.0 ,,
Streptococcus mucosus	8= 6.0 ,,
Other streptococci	28=21.0 ,,
Staphylococcus aureus	1= 0.8 ,,

It will be seen from the numbers that in about half the cases the infection was a mixed one. The list gives a fair idea of the sort of infections encountered in these cases.

Streptococci (apart from *S. mucosus*) are nearly always present in sputum, and are usually of no account; but they are sometimes of real importance, especially when oral or tonsillar sepsis has been a factor in establishing the bronchitis.

The striking successes occur mainly with pneumococcus, influenza bacillus, Friedländer's bacillus, and *S. mucosus*. The *M. catarrhalis* appears in several forms, of which the commonest seem to play an unimportant part in the causation of bronchitis, but the type which grows in soft colonies and is haemophilic is always worth using in a vaccine. I am quite sure that the bacteriologist who does not grow pneumococci and influenza bacilli in a good proportion of his cases of bronchitis is guilty of some technical fault and will not get really good results from his vaccines.

The diphtheroid bacilli (*B. septus*, etc.) are frequently present, but I have never been convinced that they are of importance, and therefore do not use them as vaccines. There are several types of Gram-positive diplococci or diplostreptococci which are as common in the sputum as in the saliva, and might be mistaken for pneumococci under the microscope, but in culture they can deceive only the inexperienced. I know nothing about filter passers and ultramicroscopic germs, and can only hope that if the day comes when they are proved to be the real cause of respiratory infections it will also be proved that they find their way into our vaccines along with the germs we now consider important.

There is no need to say more about the preparation of autogenous vaccines from sputum. Its success depends largely on obtaining a fresh warm sample, free from contaminations with mouth organisms and food. In favourable circumstances it may still be necessary to test several specimens from the same patient before one gets satisfactory cultures. There are occasions, however, on which it is impossible to obtain a suitable specimen of sputum—for example, in:

1. Small children who cannot be taught to expectorate.
2. Adults who have just recovered from "another bad attack" and wish to avoid further attacks, but have no sputum.
3. Patients who have all the signs and symptoms of bronchitis except that they cough up no sputum or only a little clear mucus devoid of useful bacteria.
4. Patients who as the result of bronchitis are left with a most irritating dry cough.
5. Patients who have developed asthma as a direct sequel to repeated attacks of bronchitis.

It was in trying to deal with a case of severe chronic bronchitis in a little girl that I discovered a test which has since proved of the greatest help in nearly all subsequent cases. Finding it impossible to obtain any sputum, I took cultures from her nasal passages, and obtained such a pure and profuse growth of pneumococcus that I was able to prepare a vaccine from the primary cultures. Her doctor carried out the inoculations and reported rapid and complete recovery.

After that I tested the nasal passages in every case, and soon discovered that there is a definite connexion between the infection of the nasal passages and the liability to bronchitis. From an experience of 300 cases of bronchitis dealt with during the past eight years I can state that:

1. In many cases the nose and sputum show exactly the same infection, but the nasal cultures, being purer and more profuse, are usually more suitable for the preparation of vaccines.
2. In other cases the nose and sputum show different infections, yet both are important—for example, the nose shows pneumococcus, the sputum influenza bacillus.
3. In other cases the nose shows a useful infection, while the sputum shows none.
4. During free intervals the patient frequently carries in his nasal passages the infection which causes his bronchitis.
5. In most cases in which there is no sputum a convincing culture can be obtained from the nasal passages.
6. Vaccine treatment becomes much more successful when cultures from the nasal passages are used, combined, when necessary, with those from the sputum.
7. There is bacteriological and clinical evidence that bronchitis is not a primary disease but an extension or result of a nasal infection. As a rule such patients do not complain of nasal catarrh, though it is not unusual to see muco-pus coming from the back of the nose into the pharynx.

The following are some of the nasal findings from cases of bronchitis. To those who believe that normal healthy nasal passages abound in pathogenic germs these findings will make no appeal. In my own experience profuse cultures of the chief catarrhal germs are rarely obtainable from healthy persons, and I beg that clinical bacteriologists will give the nasal test a fair trial before condemning it.

The nose was tested in 276 cases of bronchitis and was negative in 20. The 256 positive cases showed:

Pneumococcus	in 118
M. catarrhalis	,, 82
B. influenzae	,, 49
Friedländer's bacillus	,, 38
S. mucosus	,, 20
Streptococcus	,, 29
Staphylococcus aureus	,, 15
Staphylococcus albus	,, 7

In no fewer than 173 cases the vaccine was made from the nose alone, either because the sputum was negative or because there was none to be had. The bacteria found and so used were:

Pneumococcus	in 73
M. catarrhalis	,, 60
B. influenzae	,, 30
Friedländer's bacillus	,, 22
S. mucosus	,, 14
Streptococcus	,, 18
Staphylococcus aureus	,, 11
Staphylococcus albus	,, 7

Note the similarity between this list and that of the germs found in the sputum of 130 cases.

On the whole these vaccines were so successful that I am forced to the conclusion that the cause of bronchitis, and also of some types of asthma, is to be traced to an infected state of some part of the nasal passages.

The method which I use for testing the nasal passages is very simple:

1. Plates of blood-agar are prepared and warmed in the incubator.
2. The patient sits with head upright facing the light.
3. A tiny sterile swab of cotton-wool mounted on thin wire is gently introduced into one nasal passage, passed horizontally

backwards until it reaches the posterior wall of the nasopharynx, and withdrawn after being left there for a few seconds. The other side is swabbed in the same way.

4. Without the slightest delay each swab is carefully rubbed on a warm blood-agar plate, which is placed in the incubator.

Let it be made perfectly clear that this test can only be performed with the patient near the incubator; if it is not done in this way it ceases to be the test on which this paper is based.

It happens sometimes that the patient is not well enough to come to the laboratory, and in that case the incubator and media must go to the patient. For this purpose Messrs. Chas. Hearson and Co., Ltd., of Regent Street, London, have made me a little warm box just large enough to hold a few Petri dishes. Its water-jacket can be filled with warm water at the patient's house and will remain at about body temperature for several hours, long enough to get the cultures safely to the laboratory.

Bacteria such as pneumococci and influenza bacilli will usually live for some hours in warm sputum, but they are likely to die on a tiny swab even if it is kept warm; hence the advisability of making cultures immediately. Such cultures often show such a profuse growth of pneumococcus, influenza bacillus, Friedländer's bacillus, or *S. mucosus* that a vaccine can be made immediately; but if in small numbers any of the above germs are worth subculturing and using.

Diphtheroid bacilli are very common in the nose; rightly or wrongly, I have never attached any importance to them nor used them in a vaccine. I have also ignored small growths of *M. catarrhalis*, streptococci, and staphylococci, but profuse pure growths are of some importance and should be used.

It is instructive to compare the result of cultural examination of the sputum with that of the nasal passages in 128 cases in which both examinations were made.

Table showing Percentage of Various Organisms found on Cultural Examination of Sputum and the Nasal Passages of 128 Cases.

	In the Sputum.	In the Nose.
Pneumococcus	30.0	47.0
M. catarrhalis	27.3	23.5
B. influenzae	34.0	20.0
Friedländer's bacillus	7.0	15.0
S. mucosus	4.7	6.25
Streptococcus	17.0	10.0
Staphylococcus aureus	1.0	4.0

Surely these findings show an extraordinary similarity between the bacteriological state of the bronchi and the nose; one is forced to one of three conclusions—either that these catarrhal germs have nothing to do with bronchitis (in which case we must explain the good effects of vaccines by assuming the presence therein of something more than the known catarrhal germs), or that the nasal passages are infected from the bronchial tubes, or the bronchial tubes from the nasal passages.

I do not hesitate to believe that the last explanation is the correct one, seeing that it is an everyday occurrence for a cold in the head to spread to the chest, that nasal cultures are purer and more profuse than those from the sputum, and that the nasal infection persists during the intervals of freedom from any signs of bronchial infection.

All the patients upon whose cases this paper is founded have been sent to me by their doctors, who have given the inoculations and reported their effect.

I have not made vaccines for patients when I found the bronchitis to be associated with advanced renal and cardiac disease, tuberculosis, or mediastinal tumours. Examples of bronchiectasis are not included in this paper.

The aim of the treatment is to cure those who suffer from frequent attacks but are free during the intervals, to relieve cough and asthma and reduce expectoration in those cases in which a cure is obviously impossible, and to improve the general health and reduce the liability to and severity of acute exacerbations in all.

The vaccines are sterilized by heat, their strength estimated chiefly by the amount of culture used and checked by a count of a stained film. Fifteen doses are prepared and given at intervals of a week, starting with a small dose but increasing steadily as long as the patient gets no unpleasant reaction. Such a course appears to confer a definite immunity lasting a year or more in the case of those who continue to be carriers, and for an indefinite period in the case of those who cease to be infected.

A vaccine does not succeed because it rids the patient of his germs, but because it renders him unassailable for a certain period; during this time his symptoms may clear up completely though he may continue to harbour his germs in undiminished numbers.

Conclusions.

Recurrent and chronic bronchitis can be cured or relieved by treatment with autogenous vaccines.

When sputum is available it should be sent immediately by messenger to the bacteriologist, and not allowed to get cold.

From such a sample it may be possible to make a good vaccine.

Patients suffering from bronchitis usually have an infection of the nasal passages, which is largely responsible for the bronchitis.

Cultures made from these nasal passages according to the method described in this paper show the same sort of germs as are found in the sputum and make better vaccines.

Nasal cultures should be made in every case of bronchitis in which a vaccine is required, and cultures should be made from the sputum also when it is obtainable; in this way one gets a true picture of the underlying bacterial infections and can prepare a more efficient vaccine than if sputum alone is used.

When no sputum is obtainable a successful vaccine may be made from the nasal cultures alone.

Vaccines do not effect a permanent cure, except in those who cease to carry their nasal infection. But in carriers they confer a useful period of immunity.

It is probable that in many instances a rhinologist could throw light on the real nature of these associated nasal infections and suggest some useful local treatment. In the majority of cases the persons so infected are not conscious of any nasal catarrh, though a post-nasal discharge is often obvious to the physician.

The 300 cases upon which this paper is built were just ordinary private patients sent by their doctor to consult me, there was no preconceived plan or idea in the investigations, which extend over eight years, and any merit these observations possess is due entirely to the painstaking work of my two bacteriological assistants.

SURGERY ON THE HIGH SEAS.

BY

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AND

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The following is a short account of an operation performed on board ship.

The patient, a pantryman of the ship's crew, an old and seedy-looking man of 70, reported sick with retention of urine. At the age of 25 he had contracted gonorrhoea, and showed a healed chancre on the glans penis. For nine months previously he had been troubled with frequent and difficult micturition. There had never been any pain or haematuria, but for the last three months there had been constant dribbling.

He was pale, anaemic, and very wasted. His vessels were very thick and atheromatous, the liver was small, the superficial abdominal veins dilated, and the tongue was furred and dry; he complained of headache. There was no tenderness over the kidneys, but the bladder reached up to his umbilicus. The urine was pale, specific gravity 1004, acid; it contained no blood, but showed a slight haze of albumin, and had a somewhat fishy odour, suggestive of *B. coli* infection of the urinary tract. For three days previously he had vomited at night. On rectal examination the prostate was found to be irregular and definitely enlarged, more so on the right side. It was hard but not fixed.

For ten days his condition was relieved by the passage of a silver prostatic catheter. There was always difficulty in passing the instrument through the membranous and prostatic portions of the urethra. It would stick and then suddenly plunge into the bladder with a corkscrew-like action, but no sense of grating was ever detected. Finally one morning it was impossible to pass the catheter, and suprapubic cystotomy was performed.

The operating theatre was a double-bunked sick bay right over the propeller. It was a rough windy day. The stern rolled and pitched vertically up and down, and to this was added a lateral vibratory movement when the propeller came up into surface water. An electric hand torch proved very useful, as the light was cut off by the fixed bunks overhead. The patient being considered too bad for a general anaesthetic, stovaine-billon was successfully injected intrathecally. No rubber tubing being availab e, a 1½ in. bore hose-piping, sterilized and stitched into the bladder, proved an efficient if not too liberal means of drainage. An improvised Hamilton Irving receiver was made out of an inverted soap dish.

All the holes were plugged with cork except one in which a thin rubber tube was inserted to siphon off the urine.

The patient stood the operation well, but died two days later, within sight of land.

A modified *post-mortem* examination was performed. The aorta showed very marked atheroma, the lumen being almost obliterated. The liver was small and adherent to the anterior abdominal wall. The left kidney was small and tough, most of its substance being flattened by a large hydronephrotic sac. The right kidney was larger, with a lesser degree of hydronephrosis than the left. The bladder wall was thick and trabeculated. The prostate consisted of a mass of small stones embedded in tough fibrous tissue with practically no gland substance present.

As a sequel to the *post-mortem* examination, which of necessity was performed without gloves, the operator pricked himself in sewing up the wound. No local reaction asserted itself, but within twenty-four hours he experienced "shivery muscular pains," which were followed by a rise of temperature to 103°, and profuse sweating, on three successive evenings, and then the passage of pus in the urine. The latter showed *Bacillus coli* on bacteriological examination.

NINETIETH ANNUAL MEETING

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PROCEEDINGS OF SECTIONS.

SECTION OF MICROBIOLOGY (INCLUDING BACTERIOLOGY).

ROBERT MACNEIL BUCHANAN, M.B., F.R.F.P.S., President.

DISCUSSION ON SOME SIMILARITIES AND DISSIMILARITIES BETWEEN PLANT AND ANIMAL DISEASES,

WITH SPECIAL REFERENCE TO IMMUNITY AND VIRUS DISEASES.

OPENING PAPER

BY

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IN common with the study of the human diseases due to foreign organisms, the study of the parasitic diseases of plants has made very great strides during the last few decades. The question, then, naturally arises as to how far the discoveries made in one of these fields are of service in the other—that is, as to how far comparative pathology of animals and plants is a help in future work. On the animal side the greatest advances have been in two fields—those of the exact determination and characterization of the causal organism, and of the physiological study of the basis of immunity. From the last has arisen the vaccine and serum therapy which are the most striking triumphs of modern animal bacteriology. On the vegetable side also very great advances have been made in the study of the etiology of disease, and a very large number of parasitic fungi and bacteria have been described. In the field of disease resistance, however, the advance on the vegetable side no longer runs parallel with that of the animal. Our knowledge of the physiological basis of the resistance to disease of plants is still very rudimentary, and our methods of disease control have been directed mainly to the prevention of the entry of the foreign organism into the host plant, rather than to the destruction by a reaction of the plant body of the organism after invasion.

One of the difficulties that face the plant pathologist in any attempt at broad generalizations concerning parasitic diseases is the very great range in the degree of closeness of association between host and parasite. In the case of most bacterial diseases of man the parasite is definitely internal and produces a marked physiological reaction. In the case of fungal diseases of plants, however, we find very various degrees of closeness of association between parasite and host. We have the simplest cases where the fungus lives solely on the surface of the leaves, sending down no sucking organs into the host tissues and absorbing no nourishment directly from the host. The injury done is purely mechanical by blocking the stomata and intercepting the light which the leaf requires for its proper activity. Again, we have parasites which live mainly on the surface of the host, yet send down minute absorbing organs into the tissues below. Then there are parasites which confine themselves to the intercellular spaces of the plant, living between and outside the cells and obtaining their nourishment by the diffusion of nutritive substances from the host cells with which they are in contact. A closer degree of association is also to be found in which the main body of the parasite, as in the case just described, lies inside the plant body, in the intercellular spaces outside the cells, but special absorbing organs are present which penetrate the host cells and absorb nourishment from them. Finally, we have parasites like those of wart disease of potato, and finger-and-toe disease of cabbages, which live entirely within the host cells. The degree of structural association of host and plant parasite is thus seen to exhibit a wide range of complexity. The angle from which the plant pathologist can obtain the most illuminating view of the problems which confront him is, however, not the structural but the physiological. The plant pathologist, like the animal pathologist, is mainly concerned with the effect of the parasite on the function of the host—that is, with the interrelation of the physiological processes of the host and the parasite. Disease is abnormal physiology, and its further analysis lies in an adequate knowledge of the physiological processes of the foreign organism and of the plant on which it is parasitic. Just as we find various degrees of closeness of anatomical association, so we find various degrees of physiological relationship.

The different physiological classes exhibiting these various degrees of association are numerous and merge insensibly into one another. At one extreme we have the well-known parasite *Botrytis cinerea* (producing a rot of grapes and numerous other plants), where the relations of host and parasite are comparatively simple and well understood. *B. cinerea* is a very common fungus and is able to live as a saprophyte on dead material as well as parasitically. A tissue attacked by the organism is rapidly killed and reduced to the condition of a soft mush. A closer examination shows that the death of the host cells occurs at some distance in front of the advancing threads (hyphae) of the fungus. This action at a distance is brought about by the diffusion from the fungal threads of a powerful enzyme which dissolves the interstitial substance between the cells and brings about their separation and death. This enzyme can be extracted from an active culture of the parasite when grown on artificial media, and its injection into the host plant will bring about all the phenomena of disintegration and death associated with the attack of the living fungus. There is one marked difference, however: the spores of the fungus, when placed under proper conditions on the *outside* of the leaf of an appropriate plant, can enter and bring about the disintegration of the tissue. An extract containing the enzyme has, however, no action when placed on the surface of the leaf or stem; it is necessary to inject it into the intercellular spaces before it can exert any destructive action. The difference is due to the protecting layer of cuticle which covers the surface of the aerial parts of the plant. Upon this cuticle the enzyme has no action and it cannot diffuse through it, so that it is only when this layer is passed that the fungus can exert any lethal action.

The cuticular barrier is found to play a very important part in many cases of disease resistance. The germ tubes of a parasitic fungus which infects the cuticle-clothed stem or leaf must either force the cuticular barrier directly or turn the barrier by entering the plant through the stomata. We find, then, that the question of penetration through uninjured surfaces plays a larger part in the plant diseases than in animal diseases, where entry through the various body opening is comparatively easy. Whether the fungus chooses the easy course through the stomata or the more difficult passage through the epidermal cells is generally characteristic of the fungus, or at least of a particular stage in its life history.* It has been pointed out above that the enzymes of *B. cinerea* have no action on cuticle, so that the passage of the fungus through the cuticularized epidermis of the host

* Why so many fungi should find their way through the cuticle when entry by the easy part of the stomata is open to them is still an evolutionary puzzle.

would seem somewhat surprising. It has been shown, however, of recent years, that the tip of the germ tube, which is developed from the spore of this fungus, becomes closely adherent to the surface of the host plant, and from this germ tube a narrow infection tube is put out which bores its way by mechanical pressure through the cuticular layers of the wall of the epidermal cell. Once these layers have been pierced the enzymes which diffuse from the advancing infection tube can dissolve the cell wall and kill the cells, and infection is complete. The same method of penetration has been observed in a number of fungi. It is obvious that if the cuticle is thick and resistant the mechanical pressure—exerted by what are doubtless the osmotic forces of the germ tube acting against the forces of adhesion holding the tube to the host surface—may be insufficient to penetrate the cuticle, and the host is then endowed with disease resistance to this particular fungus. In a case of this kind, where the physiological relation between the host and the fungus is of the simplest, there appears to be no host reaction with which the parasite is concerned. If the fungus can enter and the cell wall is of a chemical nature to be acted upon by the enzymes of the fungus, then infection occurs and the tissues are killed.

Standing, in comparison with *Botrytis*, at the other extreme of physiological interdependence, we find—to take two examples—such parasites as the "rust" fungi, which do so much damage to cereals, and the organism causing wart disease of potato. In the rust fungi we find the question of entry into the host plant is not of so much importance, for the fungus to a large extent enters through the stomata, the cells being pierced by the threads of the fungus after they have gained the intercellular spaces; the cell walls here perforated are not protected by a cuticle. It is after the cells of the host are penetrated by the absorbing organs (haustoria) of the parasite and infection is established that the most striking difference between the rust fungus and *Botrytis* is established. In attack by the rust fungus not only is there no killing in advance of penetration nor even any apparent injury to the cells actually entered by the fungus, but the cells are stimulated, at least at first, to active growth and development at the same time as the fungus is nourished and multiplies in the tissues. We have thus established an association of host and parasite by which, at least for a time, both of them profit—an association to which the term "symbiosis" is applied. One of the difficulties in analysing the nature of plant parasitism results from the way in which parasitism merges into symbiosis in the case of many of the more highly specialized fungi. In the rust fungus the symbiotic relation is not a permanent one, but passes over into definite parasitism, for the leaf cells which have been entered by the fungus and stimulated to active division are ultimately killed.

In resistance to attack by rust fungi, in which entry is by the stomata, protection by the cuticle can, of course, play no part; in fact, it has been shown that the germ tubes of the rust fungus can enter the tissues of almost any plant, but they can only successfully infect their proper host. The question of the rust resistance of different cereals and their varieties is of great economic importance, and fortunately we have some knowledge of the nature of this rust resistance. It has been shown that if the spores of rust fungi are sown on the leaves of cereals which they do not normally parasitize, the germ tubes developed from the spores enter the stomata as usual and the infection hyphae come in contact with the leaf cells. Instead, however, of contact being followed by penetrations and the normal production of absorbing organs, a very marked reaction is produced in the host cells. They rapidly die without being penetrated, and the fungal hyphae die also and so infection is stayed. Immunity to disease in these cases depends not on any active resistance to attack but on a marked *hypersensitiveness* of the host cells, so that they are killed by mere contact with the parasite, and no symbiosis is established. There has been some suggestion that we have here a play of toxin and antitoxin, but there is no evidence of this; we find no attack followed by recovery, but both the invader and the attacked cells succumb in the contest. In the present state of our knowledge of cell dynamics we cannot analyse the phenomena further. All that we can say is that in this class of plant disease we have in the one case a *harmony* between the physiological processes of the parasite and the cells of its normal host—a harmony leading to the symbiotic relationship which is the necessary preliminary to the parasitic relationship—while in the other case there is a *disharmony* between the physiological processes of the would-be parasite and its host, leading both to the death of the parasite and of the localized groups of host cells, and thus to escape from disease. The case of resistance to rust attack just described is an interesting example of immunity due to definite localized physiological reaction of the host, but it is widely different from the physiological reactions by means of which immunity is acquired in animals.

Another type of physiological reaction to infection on which immunity to many plant diseases seems to depend is the well-known wound reaction leading to the development of cork. The different varieties of flax, for example, show very varying degrees of resistance to infection by *Fusarium lini*. The difference appears to depend on the fact that the parasite enters the tissues without difficulty, but in the case of the susceptible host plants there is little or no wound reaction, so that the fungus can make almost uninterrupted progress through the tissues; with the variety showing resistance the entry of the parasite is followed by a marked reaction, leading to the production of cork, which surrounds the invader with a barrier of dead and impermeable cells so that it is either completely starved out or else its progress is markedly checked.

The case of *Botrytis*, where the parasite can be left out by anatomical peculiarities (such as a thick cuticle), gives us an example of what may be termed passive immunity in plants, while the two cases mentioned, where resistance depends on some physiological reaction of the plant, may be termed active immunity, though the terms have a different significance from that in which they are used by animal pathologists. All cases of active immunity cannot be explained on the basis of *hypersensitiveness* or on a definite wound reaction. Other physiological conditions play a large part, but exactly what those conditions are it is at present impossible to say. Higher osmotic pressure of the sap, and hyperacidity or tannin contents of the cell, have been claimed by various workers as the basis of the distinction between resistant and susceptible forms, but such theories are built on very insecure foundations. The differences, as indicated above, would seem to depend on more recondite physiological differences of cell metabolism, which cannot at present be analysed.

It will be seen that the immunity with which the plant pathologist is concerned is of the nature of natural immunity, the physiological basis of which in animals is still very obscure. The acquired immunity resulting from one attack of a disease—the immunity which is so characteristic of many diseases of man, and is the basis of modern serum therapy—is quite unknown in relation to definite plant diseases. This is perhaps the most striking difference between plant and animal pathology. The difference between the animal and the vegetable kingdom is to be correlated with the absence of any general reaction to disease of the plant body as a whole. The plant body is only very slightly integrated, and the various plant organs have a great degree of independence, as is shown by the fact that a new plant can often be "struck" from a shoot, a root, or even a leaf. In agreement with this we find such reactions to infection that occur are markedly localized; each group of attacked cells fights a lone battle in which it is largely independent of the rest of the plant body. It is impossible to say that in such a contest between host and parasite there is no production of toxins and antitoxins, or similar substances, but there is no clear evidence of such production, and even in the case of animals the production of antitoxins and other substances associated with acquired immunity seems to be confined to warm-blooded animals. Even if such substances are produced by plants, the chances of isolating them would seem very meagre, for the infection in the plant generally progresses somewhat slowly, at any given moment only a small group of cells being attacked; and, furthermore, there is no reservoir corresponding to the blood stream of animals from which they could be extracted and studied. This being so, it is certainly unlikely that serum therapy can be applied to combating the diseases of plants. Even if such serums could be prepared, there would be the difficulty of distributing such substances, probably colloidal in nature, throughout the plant in the absence of a circulatory system. Even if such distribution were accomplished, there would be the further difficulty of the continued embryonic condition of the plant. The plant is continually producing new organs, so that any serum therapy to be effective would have not only to endow the plant body with acquired immunity, *but with an immunity which would be transmitted to the newly developing organs*. The experience of animal pathologists concerning the inheritance of acquired immunity is certainly unfavourable to any such expectation.

An interesting point of contact between plant and animal pathology is in the so-called "virus" diseases. Plant pathologists have of late years become familiar with a number of very definite diseases of a highly infectious nature, in which, however, all attempts to isolate a definite organism have proved of no avail. The first of these diseases to be closely investigated was the mosaic disease of tobacco, a serious disease of that crop in which the characteristic symptom is the development of light green or yellow patches on the leaf; these patches, being interspersed with patches retaining the normal colour of the leaf, give a mosaic appearance to the leaf. The disease is exceedingly infectious; a drop of the sap of a diseased leaf placed on the injured surface of a healthy leaf will cause infection. The sap can be diluted many hundred times and still be effective, and the "virus" is found to pass a Chamberland filter.

The older workers unfamiliar with the idea of ultramicroscopic parasites put forward various theories in an attempt to explain infection in the presumed absence of living organisms. The cell sap was supposed by one worker to contain a *contagium vivum fluidum*—that is, some kind of "living fluid." Other workers fell back on the view that the disease in question was an *enzyme disease*, due to an enzyme which was transferred from plant to plant and had the power of increasing when so transferred to an appropriate plant. It has been shown, however, that although the virus passes a Chamberland filter it can be held back by a porcelain filter of suitable porosity, and by a layer of powdered talc, both of which allow the passage of the enzyme with which the virus is associated. There is thus evidence that the virus is particulate in nature; and as, furthermore, it is found to be destroyed by heat and by antiseptics, the hypothesis which explains the facts with the greatest economy of assumption is that of an ultramicroscopic organism. During the last few years quite a large number of virus diseases have come to light. The mosaic disease of potato and the leaf-roll of potato, which are serious diseases in this and other countries, are both of this class. Mosaic diseases are also found on other plants—as cucumber, tomato, sugar beet, sugar cane, maize, clover, etc.; they appear to be increasing in seriousness at the present time.

A very interesting discovery has been made since 1920 by investigators working in Holland and in the United States. It has been shown both in the case of mosaic and leaf-roll of the potato, in common with similar virus diseases of animals, that these two diseases are insect-borne. By an investigation lasting over several years, including careful feeding experiments and the use of insect-proof cages, it was conclusively shown by two Dutch workers that both leaf-roll and mosaic were carried by aphides (green fly), and at present as far as is known the diseases are transmitted in no other way. The incidence of the disease is thus closely related to the time of development of the aphides and the degree to which the plants are infested.

These virus diseases also show an interesting analogy with animal diseases. Leaf-roll, for instance, can be transmitted by grafting, and when some members of the same family as the potato are infected by grafting they show no symptoms of disease, but if they are grafted on healthy potato stocks they cause these to become infected. The same phenomena can be observed with mosaic disease. The plants in question are thus able to transmit the disease without themselves showing symptoms of it; they are thus of the nature of carriers.

In this brief survey many interesting aspects of plant disease have had to be neglected, such as the phenomena associated with infection of the seedlings of orchids, but enough has been said to indicate that though the parasitic diseases of animals and plants have many points in common, yet the dissimilarities are on the whole more marked than the resemblances.

DISCUSSION.

Mr. J. Ramsbottom (British Museum, Natural History) said: I wish to lay stress on certain aspects of immunity as met with in the problem of symbiosis. The term "symbiosis" is much abused in biology; in these remarks it is used simply to denote the constant association of two organisms. The relation between the two is doubtless always one more or less of antagonism, and probably the state is a sign that neither opponent gains the complete mastery. It cannot be too clearly emphasized that the relation between the two organisms in each particular case is a separate problem. One of the best known examples of symbiosis in plants is the association of fungus filaments with the roots of the majority of flowering plants forming what is termed a fungus root or mycorrhiza. Two types may be distinguished—the one in which the fungal threads form a sheath round the tip of the root (ectotrophic), the other where the fungus enters the cells of the root (endotrophic); the former is best known in forest trees, the latter in Orchidaceae and Ericaceae. Despite its etymology, the term "mycorrhiza" is used also when stem or thallus is "infected"; it is, moreover, very frequently used simply to denote the fungus partner.

Every orchid so far investigated (with the doubtful exception of *Wullschlaegeia aphylla*) has shown fungal hyphae in its roots. The distribution of these in any given root is always quite definite in a radial direction; sometimes the whole of the cortex is infected, sometimes only one or two layers of cells. The difficulty of germinating orchid seeds is one of the best known facts of horticulture. Bernard (1902) found that the solution of the problem lies in the mycorrhizal fungus. Seeds sown on a culture of the fungus extracted from the parent plant germinate freely, whereas in the absence of the fungus no germination occurs. (A similar necessity of the mycorrhizal fungus for germination has been shown to be present in *Calluna vulgaris* by M. C. Rayner.) The seeds are very small and typically are without any trace of the differentiated embryo practically universal in flowering plants. The only difference seen in the cells is that those at the end by which the seed was attached in the pod (suspensor) are usually larger than those at the other end. The fungus enters through the suspensor end and so into the larger cells. Here it rolls itself into a ball in one cell before passing on to the next. Almost immediately after infection has occurred the smaller cells at the distal end of the seed begin to undergo division and the growing point of the stem is laid down. Usually after the production of a couple of young leaves the beginning of root formation can be made out; the young root absorbs its way through the now swollen seed and enters the soil. The fungus remains restricted to the lower part of the developing seedling and never enters the meristematic tissue. Further, the root on entering the soil is free from fungus; infection takes place from the soil, usually when the young root is about a quarter of an inch long. By the time this happens the fungus in the seed has usually lost its mycelial character and become clumped. This is definitely associated with nuclear activity, the nucleus during the process becoming much increased in size and putting out pseudopodia which attack the hyphae. Similar clumping is seen in many of the cells of the root. [A series of enlarged photomicrographs showing infected seeds and seedlings was exhibited.] In the orchids studied by the late J. Charlesworth and myself no definite "digesting" cells were seen such as have been recorded by several investigators. Magnus (1900) stated that the infected cells of *Neottia* can be divided into three layers—an inner and an outer layer of "host" cells in which the fungus remains alive and is thus able to hibernate, and a middle layer of "digesting" cells in which the fungus is absorbed. In numerous cultivated forms any infected cell appears able to take on the digestive functions. Again, Bernard recorded a layer of cells in the developing seeds of *Phalaenopsis* just behind the meristematic region, which are specialized for absorption, and are recognizable by their multilobed nuclei. Such specialized cells have not been found in our investigations, where so far I am inclined to the opinion that any infected cell of the seed or of the root is a potential digestive cell. In any case we have a very clearly marked distribution of the fungus. In the seed the cells of the suspensor end alone are entered, and in the root certain cortical layers. All meristematic regions are free from infection, as also are the conducting strands and perennating organs such as bulbs. The fungus is kept away from all parts of the orchid in which presumably its presence would be inconvenient. The fungus present in orchids in all known cases (with the exception of *Gastrodia*) is always a member of the same genus which Bernard regarded as *Rhizoctonia* and for which Burgeff proposed the generic name *Orcheomyces*. Many different "species" occur which can be arranged in four or five main groups. The same species always occurs in the same orchid, though it is not usually restricted to it. There is disagreement between these two workers as to attenuation in culture. Bernard found that the fungi generally became inactive and after two years were incapable of bringing about germination, the power of an attenuated form being increased by passing it through seedlings; Burgeff (1909) found that his cultures retained their

activity unimpaired for slightly over two years. Cultures regularly renewed at Haywards Heath have retained some activity for nine years, though it is not known what their original power was. It is, however, certain that attenuated forms do occur, as is clearly apparent when the method is put upon a commercial basis.

Bernard concluded that mycorrhizal infection was a benign disease. He followed Gallaud in regarding the phenomenon of digestion in an infected cell as phagocytosis; the principle is the same, but the immobility of the phagocyte cell entails certain differences in the mode of action. The peculiar manner of growth of the mycelium in the cells is apparently related to immunity, and Bernard considered it to correspond to agglutination. It should be pointed out, however, that the balling is also frequently seen in cultures. An interesting point is that when once a hypha has entered a seed no further infection is possible; the seed is vaccinated.

Bernard also showed that the tubers of orchids contained an antibody, which is diffusible, killed by heat at 55° C., and specific. Such is a bare outline of symbiosis as it occurs in orchids. The flowering plant restricts the fungus in its distribution by the properties of its cells, either forbidding entry by chemical means, imposing a special mode of growth on the mycelium, or by causing degeneration. Such symbiosis was regarded by Bernard as representing immunity realized by phagocytosis.

Dr. William B. Brierley (Rothamsted Experiment Station) said: As particular aspects of our problem will be discussed by speakers who follow me I shall confine myself to a specific and yet fundamental question which appears to me to stand in need of re-examination. For the first few minutes I shall state platitudes, but this is necessary, for it is a direct corollary of these platitudes that I wish to bring before you. Any condition of plant disease is the *status quo* in a physiological complex; it is a physiological equilibration in which a static condition is not attained; it is a process and not an entity. The component parts of this process are the host and the parasite. Each of these is itself a physiological complex, being the outcome of a certain specific group of germinal qualities (*genotype*) developing under the moulding influences of environmental factors such as temperature, light, humidity, food, and so forth. Diagrammatically this may be shown as follows:

From such trite considerations as this has developed the concept of disease as being the invasion of a defensive host by an active parasite, a see-saw balance in which there is an inverse relationship between the health and vigour of the host and the incidence and virulence of the disease. It is unnecessary to quote definite formulations of this concept, for it is quite generally accepted, but one may perhaps indicate Adami's *Principles of Pathology*, Kolmer's *Infection Immunity and Specific Therapy*, Zinsser's *Infection and Immunity*, and perhaps more particularly Hurry's *Vicious Circles in Disease*. In phytopathology the growing of vigorous plants as a prophylactic against disease is axiomatic.

The question I would ask you is whether this concept is a true picture of reality. Are vigorous plants more resistant to disease than weakly plants, and is there a constant inverse relationship between vigour of host and incidence of disease? One may weaken plants in many ways: by a deficiency in their nitrogen nutrition and supply of mineral nutrients; by interference with their carbon metabolism, their water and temperature relations; by exposure to toxic agents; by wounding; or, on the other hand, one may experiment with plant tissues of different ages and states of maturity. Much experimental work has been carried out along these lines, and this perhaps more particularly with the rust fungi. It has been almost the invariable experience among these students —and one may instance Sheldon, Ward, and Stakman—that the inverse relationship of host vigour to disease incidence does not hold. A careful study of the evidence shows that conditions that are unfavourable to the development of the host are also unfavourable to the development of the fungus and that inoculation experiments on weakly plants usually fail, whereas those on strong and vigorously growing plants of the same variety usually succeed. The evidence, in fact, often shows a direct relationship and not an inverse relationship. It might be suggested that this is due to the special physiological conditions obtaining in the almost symbiotic relationship of rust fungus to host and that the rust diseases are a special case. That this is not so is clearly shown when the evidence in other types of disease is examined. It is most frequently found that strongly growing individuals of varieties of potato susceptible to wart disease show a higher percentage of warted tubers and a greater development of wart on the tubers than do weakly individuals of the same variety growing in the same field. Late blight of potatoes, in my experience, attacks vigorously growing plants more easily and produces greater destruction than it does in sickly plants, and L. R. Jones pointed out many years ago that heavy manuring or growth in highly fertile soil lowers the resistance of potato plants to blight and "rot." The same general direct relationship between host vigour and disease incidence and virulence has been pointed out by Marchal in the case of lettuce blight caused by *Bremia lactucae*, by McCue in the case of tomato plants suffering from leaf blight, by Peltier in the case of citrus canker, by Fromme and Murray in angular leaf spot of tobacco, by Thomas in the *Septoria* leaf spot of celery, by Levine in crown gall of beet, and by many other workers for still other diseases. I do not, however, want to leave behind my initial platitudes and go to the opposite extreme, formulating the generalization that the more sturdy and vigorous the plant the more liable is it to disease. That is an equally false concept. The standpoint that I wish to adopt is that every kind of disease must be taken on its own merits, and that at present it is quite unsafe to draw any generalizations at all. One of the most striking illustrations of this that I have seen is in the manured plots on Barnfield at the Rothamsted Experiment Station.

The conception of the diseased state as it formulates itself to me is not that of one organism struggling for its life *against* another, but as the co-ordination of the activities of two organisms, and ultimately—a far ultimate I grant—of one series of physico-chemical forces integrating with another series. It is not A B *versus* X Y, but A B *plus* X Y.

Within the limits imposed by the genotype, each complex (host and parasite) is modifiable by alteration in the environmental factors. As I see the evidence, this is not a question of biological adaptability but of the presence or absence of certain definitive and specific factors in the physico-chemical complexes—for example, an alteration in P_H value of soil or medium, of water relations, temperature, etc., the presence or absence of some particular chemical compound or element, and so forth. I venture to think that perhaps our greatest line of advance in the solution of problems of immunity, resistance, and susceptibility to disease is to attempt to restate many of our biological concepts in terms of physics and chemistry, and to make a physical, chemical, and biochemical analysis of our conditions.

May I say a few words concerning the general issues raised by Professor Blackman? In my opinion the gulf between the animal and plant kingdoms is so profound, and this particularly in the case of the higher forms whose diseases we, as pathologists, study, that to attempt to make anything more than the most vague and general comparisons is to falsify and to confuse every issue. In its morphology and anatomy, its rigid cellular structure, and in its physiological processes the plant is utterly remote from the animals and man. In the plant there is nothing corresponding with the venous and arterial systems, with the lymphatic system, with the alimentary system, and with the muscular, skeletal, and nervous systems. It is in just these characteristic features that the principal pathological responses of higher animals are seen. To my way of thinking it is not in forced comparisons of total disease complexes or in details of pathological histology and so forth that any value can accrue from the collaboration of animal and plant pathologists. Rather is it in the comparative investigation of the structure, life history, genetics, and physiological qualities of pathogenic organisms and their individual relationships to particular host complexes under standardized conditions that advances will be made; and it is in the comparative study of cell chemistry, the study of those physical, chemical, and biochemical problems which are the bedrock of disease.

Professor S. H. Gaiger (Glasgow) said: One cannot but be struck by the remarkable resemblances which at first sight there seem to be between plant and animal diseases. These resemblances, however, on closer study, become less and less apparent. So long as one confines oneself to the examination of the mere anatomical relationship

of parasite to host there are resemblances enough. When one studies the reactions of the host against the parasite the differences become marked. These differences are largely due to the absence in plants of systems actually comparable to the systems of animals. Between human and animal diseases resemblances are everywhere apparent. That is so well known nowadays that there is no need to emphasize the important bearing which each has upon the other. But between the plant economy and the animal economy there is a wide structural and physiological gulf which widens with closer examination, and so it must be with their diseases. There can be, for instance, no such thing in plants as the acquirement of immunity through an alimentary canal, and of this immunity becoming a systemic immunity through the possession of a circulatory system. The study of the virus diseases of plants is especially interesting. One bears in mind that the first known virus disease was a disease of plants. Now we find another important discovery that there is a virus disease of plants which is solely insect-transmitted. The fact that insects can play an important rôle in transmitting viruses is especially interesting to the veterinarian because it might possibly explain how mysterious outbreaks of foot-and-mouth disease arise in this country. It may be yet found that virus diseases of animals may be insect-transmitted in addition to other means of transmission.

Dr. J. C. G. LEDINGHAM (Lister Institute, London) said: Professor Blackman has confined himself to infections by the so-called higher fungi. The physiological interactions of host and fungus in plant life should therefore strictly be compared with those observed in fungal infections in animals. With this proviso I believe that the respective manifestations show a closer parallel than Professor Blackman might perhaps allow. Man and animals generally show an extraordinarily high degree of normal resistance to attack by fungi. The few representative fungal infections of man and animals are essentially dermal and characterized as a rule by great chronicity. They have little invasive power, but progress locally with accompanying solution of tissue very much as if a vascular circulation was not present. Some of these fungi also, especially those associated with various tropical mycoses, seem to attack the local tissues only after they have been weakened by certain gross physical agencies as, for example, chronic maceration. I should think that the study of the minute mechanism of these interactions which is possible in the plant may throw a considerable amount of light on similar interactions in man and animals. As we go down in the animal scale truly fungal infections of grave character appear to become more prominent, though it must be admitted our knowledge of the comparative pathology of the lower vertebrates and the invertebrate world is as yet rudimentary. It is possible, however, that looked-for transitions are most likely to be found in this zone. Professor Blackman has not alluded to the economically very important group of bacterial diseases of plants. So far as one may judge from the mere reading of descriptions of bacterial diseases in plants, they would seem to offer an extraordinarily close parallel. Though there is no true circulation, the attacking bacteria appear to exercise a considerable amount of invasive power by means of the fibro-vascular canals of the plant. Also there is a good deal of evidence that insects can act as inoculators of such bacterial diseases. As to the so-called virus diseases, we have, of course, a very close parallel in the Rickettsia group of organisms, which are intimately associated with insect transmitters, and it will be most interesting to follow up the study from the morphological side of these plant viruses in the aphides which transmit these diseases to plants. The factors determining normal immunity to disease in animals are as yet but little understood, and I agree with Professor Blackman that there is here much common ground for research.

Lieut.-Colonel F. P. MACKIE, I.M.S., said: The relationship between the protozoal diseases of plants and animals is also of much interest. To take the example of the flagellate *Leptomonas davidi* of certain euphorbiaceous plants; this flagellate is similar to, and almost indistinguishable morphologically from, the herpetomonas infection of insects and the allied parasite *Leishmania donovani* of kala-azar. These parasites are found side by side in India, and to those like myself constantly confronted with the problem of kala-azar it would be very interesting if the simpler problem in this plant could throw any light on this disastrous disease in man. The flagellate infection of the plant appears to produce disease and death in the plants affected, and is believed to be transmitted from the diseased to the healthy by the agency of parasitic insects. I sometimes feel a sense almost of despair in attempting to unravel the infinite complexities of disease in man, and think that it might be better to transfer the whole process of investigation to the simpler problems of the parasitization of the lower forms of plant life.

Dr. J. S. DUNKERLY (Glasgow) said: The parasitic flagellate leptomonads are parasites of the laticiferous system in the Euphorbiaceae, etc., and this system extends through the plant and furnishes an analogy to the animal circulatory system. According to Professor Blackman, in the rust-resistant races of wheat the host cells are killed by the parasitic hyphae, which in their turn are killed or weakened by some reaction of the host cells. This is a local reaction comparable with those produced in diseases of animals. The cells of the susceptible plants, on the other hand, are said to enter into symbiotic relations with the parasitic hyphae which attach themselves to the host nucleus. There is a somewhat similar connexion between the nucleus of a coccidian parasite, Caryotropha, and the nucleus of the cell of its host, a polychaete worm. The symbiosis of plant cells in fungal disease may be due to a relationship between the host plant and the fungal cell similar to that which exists between algal and fungal cells in lichens. This would not occur so readily between animal and plant cells, which would be more in the relations merely of host and parasite.

DISCUSSION ON MUTATION OF SPECIES.

OPENING PAPER

BY

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MUCH of the controversy waged around the subject of mutation has birth in the question of what kind of change should be regarded as a mutation; and perhaps it may not be out of place to spend a short time considering this.

"HIGHER ORGANISMS."

The recognizable characters (*phenotype*) of organisms are the outcome of interaction between germinal or hereditary potentialities (*genotype*) and external conditions. So far as there is direct evidence it appears that the hereditary potentialities are indissolubly linked with the chromosomes of the nucleus. Each potentiality may be attributed specifically to one or more particular unit factors or genes. The latter appear to be distinct and autocatalytic physical or chemical entities, perhaps ultramicroscopic particles, having definite locations in particular chromosomes. This is no hypothetical Weismannian system of "ids" and "idants" but a thing susceptible of definite proof.[26]

For our purposes phenotypic changes occurring in organisms may be grouped into two categories—*temporary* changes and *permanent* changes.

Temporary phenotypic changes are due to alterations or differences in environmental conditions and the genotype remains unaltered. In consequence these changes (*modifications*) only continue so long as the causal stimuli are operative, and disappear when the organism or its progeny are returned to the original environment. There should be no question that such changes are not to be regarded as mutations; and modifications are therefore without the scope of our discussion.

The second category comprises those phenotypic changes which are permanent—that is, character changes in organisms which persist throughout subsequent generations even when these develop under conditions in which the original stock retains the primitive phenotype. Such changes are the outcome of an alteration in genotype—that is, a change in the chromosomes of the nucleus. This category of hereditary differences may be subdivided into two groups: alterations may result from *inter-chromosome* changes or from *intra-chromosome* changes.

Inter-chromosome changes are those in which chromosomes, parts of chromosomes, or even single genes, may be distributed or recombined in various ways—much as one might shuffle and reshuffle a pack of cards. These changes fall readily into two groups: *normal* distribution and *abnormal* distribution.

Inter-chromosome distribution of normal type is the result of hybridization processes with or without the so-called "crossing over" of chromosomes and of segregation in the offspring. Now it is generally agreed that no permanent phenotypic change which is brought about in this manner may be regarded as a mutation. The latter is the result of a change in genotypic nature which occurs independently of hybridization and segregation or of "crossing over."

Inter-chromosome distribution of abnormal type is of very varied etiology. It may result from irregularities in the assortment or distribution of nuclei or chromosomes during the reduction or other divisions, from halving, doubling, tripling, and so forth, of the usual number of chromosomes, from the "non-disjunction" of particular chromosomes in the reduction division,[6] from elimination of nuclei, chromosomes, or chromatin substance during segmentation, from chromosome abnormalities, or from the actual destruction of chromosomes by specific external factors such as *x* rays,[25] and so forth. There would seem to be evidence that changes in the relative positions of genes within the single chromosome may occur, and these although strictly *intra-chromosomic* changes would need to be classed in the present category. There are some students[22] who would see in these aberrant chromosome distributions merely the finer ramifications of the normal segregation processes, but the phenomena are more usually accepted as events *sui generis*. There is no evidence in such processes of the formation *ab initio et ab intra* of new genes. Changes of this nature may occur in the individuals of what is apparently a pure line; and hereditary phenotypic changes which appear to be causally dependent upon such aberrant inter-chromosome distribution are accepted by perhaps the majority of geneticists as mutations. It is, however, vital for proof of the occurrence of such mutation that all possibility of the parental organisms having hybrid qualities be excluded; and it is being increasingly found that claims which have been made in the past, such as that of de Vries for the evening primose (*Oenothera lamarckiana*), fail when critically examined from this standpoint in the light of recent knowledge.

The second subgroup of hereditary differences comprises those genotypic alterations resulting from intra-chromosome changes. If we again use the analogy of a pack of cards, the likeness is not to rearrangement by shuffling but to an actual transformation of a card of one suit into something different. The change is of chemical or physical nature within the gene itself, analogous perhaps to the changes occurring in the atoms of the radium element. Certain prominent geneticists have urged that the term "mutation" should be confined to such an alteration in the fundamental nature of a genetic factor. To obtain convincing proof of such a chemical or physical change *in situ* is, however, an extremely difficult thing. Certainty of specific purity in the original experimental organism is obviously a *conditio sine qua non*, just as certainty of chemical purity is a *conditio sine qua non* in order to prove the existence of mutability of the elements. There are, however, data from researches on Drosophila which, in the present state of our knowledge, are best interpreted as indicating the existence of mutation by changes in the fundamental nature of genes.[27]

The point I have tried to make in the impressionistic and wholly inadequate outline I have sketched of changes in higher organisms (using this term for the whole gamut of living things from mosses and fruit flies to flowering plants and man) is that mutation is no naïve and simple concept. In modern genetical study there are obviously at least two concepts of mutation based on quite different premisses and only having in common a basis of apparently hereditary genotypic change. One concept confuses the issue by not differentiating between abnormal distribution of genes and changes in the fundamental nature of genes; whilst the other differentiates clearly but restricts the concept to a very narrow and, at present, an almost theoretical issue. The former use of the term "mutation" is much as though a chemist were to class together the refining of silver from a lead ore containing silver and an actual transformation of the element lead into the element silver.

To obtain satisfactory proof of the occurrence of mutation, using the term in either sense, it is obviously necessary, first, to be minutely acquainted with the genetical constitutions of the parent and the "mutant" organisms, and this can, in our present state of knowledge, only be elucidated by cross-breeding experiments; secondly, to have knowledge of the chromosome structure of the nucleus; and thirdly, to make free use of Occam's razor.

I have, perhaps, lingered overmuch on this first portion of my subject; but if this discussion is to have any value, and if progress is to be made toward the solving of our problems, it is essential that we get our ideas and criteria very clearly in focus. Let me now examine the question of mutation in micro-organisms from the point of view I have suggested.

The three groups of micro-organisms that concern us here are the protozoa, the fungi, and the bacteria.

PROTOZOA.

It was early shown that if one experimented with a number of wild forms, subjecting them to selective influences, hereditarily different strains could be bred out; and for some time this kind of work was accepted as demonstrating the efficacy of selection in producing "mutant" strains. It was, however, gradually realized that the phenomena were not quite so simple as they seemed, and that the results obtained, far from demonstrating the production *ab initio* of new strains, only showed that selective treatment could isolate different strains pre-existing in an original mixed population.

Johannsen's *pure-line* hypothesis was then adopted, and more accurate and controlled work carried out, the experiments being based on cultures derived from single individuals of known genealogy. In practically every reliable case entirely negative results were obtained.

However, within the last five years or so, investigations by Jennings,[16] Banta,[3] Jollos,[20] Hegner,[13] and others, have shown that very rarely permanent hereditary changes do arise in what are apparently pure-line stocks. Are these changes to be regarded as mutations? The answer depends, as we have indicated, upon the etiology of such changes, but, unfortunately, of their genetic causation we are yet singularly ignorant. Many of the subject organisms show the form of sexuality known as conjugation, but no student of the protozoa has yet thrown light on the genetic constitutions of individuals by cross-breeding; and of the genetic details of their chromosome structure we are still ignorant. The only basis for the assumption of initial purity lies in the facts that the experiments are carried out on the progeny of single organisms; that self-fertilization over a number of generations in the original stock gives mathematical presumption of a relatively high percentage of homozygous individuals, of which one has been chosen as the progenitor; that in the experiments reproduction is uniparental; and that in parallel cultures the "mutation" does not occur. Anything more critical than this is at present beyond our reach; but this is not a sufficiently critical basis on which to claim proof of the occurrence of mutation, for the crucial test of genetic analysis by interbreeding is absent.

Furthermore, our ignorance of the relative genetic significance of the micro- and macro-nuclei in protozoa (an amicronucleate Oxytricha has been found!), of the nuclear rearrangements in the normal processes of conjugation, and even more of endomixis, is profound. Recently it has been shown that certain of the genotype changes in Arcella are merely the result of a simple increase or decrease in number of nuclei,[14 23] whilst Erdmann's work[10] indicates that genotype changes in Paramecium may be due to the formation of new strains by normal recombinations of hereditary units in the process of endomixis. These changes would therefore have no greater genetic value than those occurring during normal segregation processes, and all that has been done in the experiments is to separate out hereditarily diverse strains from a mixed population. It follows that a single individual culture is not necessarily a pure line, an understanding that cuts away the very foundations of previous genetical work on the protozoa. It would thus seem that in the protozoa we cannot yet differentiate between genotype changes due to normal chromosome distribution, which are not mutations; changes due to aberrant chromosome distribution, which are only to be regarded as mutation if this term possesses a vague and indiscriminatory significance; and mutations in the more exact sense, resulting from a change in the fundamental nature of one or more specific hereditary units. Even in saying this we are perhaps going too far, for it implies that the hereditary mechanism in the protozoa is in behaviour and genetic structure comparable with that in higher organisms. It may be so, but there is yet no definite evidence that the protozoal mechanism is susceptible of exact factorial analysis as, for example, is that of Drosophila.

What I wish to point out is that mutation-(in either sense) may occur in the protozoa, but there is yet not one iota of evidence which compels us to believe that it does. Further, that to equate the concept of mutation as it is commonly

applied in protozoology with the several concepts of mutation accepted in the genetical study of higher organisms is to falsify and to confuse the issues.

FUNGI.

Mutations in fungal species have been described by Schiemann,[29] Waterman,[32] Blakeslee,[4] Thom,[31] Burger,[2] Schouten,[30] Haeincke,[12] and many other investigators. The procedure by which the new forms have been obtained has been to subject, usually but not always, a single-spore culture or its progeny to various physical or chemical conditions, to observe that rarely some entirely fortuitous change occurs in sporadic portions of the growth, to isolate the new form of organism, and to prove that in occasional instances it breeds true to type. This derivative form apparently differing hereditarily from the parent stock is thereafter referred to as a "mutant."

In no case have cross-breeding experiments between the "mutant" and the parent form been carried out, nor is this possible as yet with many of the organisms concerned; and in no case has a cytological study been made of the "mutants." In brief, we are in every case totally ignorant of the genetic constitutions and chromosome relations of the original and derived forms; and there is no certainty of specific purity in any single one of the initial organisms. We can therefore have no knowledge of whether the new form has arisen by normal processes of nuclear rearrangement, by aberrant chromosome distribution, or by changes in the fundamental nature of specific genes. It would seem, therefore, somewhat premature to use the term "mutation" in mycological studies when all that there is evidence for are apparently hereditary changes of unknown genetic causation.

A few words further may be said concerning the possible etiology of so-called "mutants" in fungi. One of the great lines of progress in mycology during the last two decades has been the demonstration of sexuality *in esse* or *in posse* in an ever-increasing number of fungi; and he would indeed be a rash mycologist who would assert that sexuality is completely absent from any single species. We have therefore to take into consideration the probability of heterozygosis in many forms and the possibility of it in all; which implies that a single-spore fungal culture has neither greater nor less genetic value than a single-seed or a single-tuber culture derived from some probably or possibly cross-bred higher plant of unknown genetic constitution. No geneticist would accept the latter as a sound basis for any kind of experimental work unless by long and careful breeding-analysis he could unravel its genetic constitution and obtain progeny homozygous for the particular character he wished to investigate. Such factorial analysis is as yet quite impossible in the fungi.

Furthermore, in perhaps a majority of fungi we are not dealing with organisms having simple uninucleated cells; but, as in the Phycomycetes, with individuals each of which consists of a single cell containing innumerable nuclei; or, as in very many Ascomycetes and Fungi Imperfecti, with multicellular individuals each cell of which may contain a very variable number of nuclei; of two adjacent cells in a hypha, one may contain five nuclei and the other seventeen or any other number. The possibilities of normal rearrangement of hereditary units or complete nuclei in such a condition are immense and practically unexplored. The studies of Burgeff[3] and Orban[28] on sexuality in Phycomycetes have, for example, shown the probability, not of a heterozygous condition, but, what is even more startling, of a *heterocaryotic* condition; and the normal genetic ramifications of the permutations and combinations of hereditary units in such a state leave one's mind almost bemused.

Again, practically all fungi that have been carefully observed show vegetative anastomoses of hyphae. Matsumoto[24] has just recorded for *Rhizoctonia solani* a phenomenon that I have frequently observed in *Botrytis cinerea*, and which I have no reason to doubt is present in many other fungi—namely, fusions between the hyphae of different strains. The possibilities of genetic contamination in this process are such as to render certainty of specific purity in any initial subject organism quite out of the question.

Practically all fungal species which have been intensively studied reveal themselves to be a congeries of strains differing from each other in a greater or less degree. Most such strains are admitted by all investigators to be constant, but occasionally a strain is studied which gives rise to forms differing from itself but similar to strains occurring wild in nature, and these "new" strains are sometimes found to be constant. Now, I would suggest that this phenomenon is not one of mutation, but that the "mutating" parent organism is genetically impure, being either heterozygous or heterocaryotic, and that its component genetic forms, or different nuclear or chromosome combinations, may occasionally be segregated out by perfectly normal processes of fungal cell-division and reproduction. Occasionally such segregants arise in conditions which favour their reproduction, and, if we chance to observe them, we call them "mutants." If segregation is complete the new form is homocaryotic or homozygous, and naturally, if isolated, remains constant. If segregation is incomplete, the new form may "revert" to the parental type, or at some later date may again show segregation.

Furthermore, it will be evident that in any fungus which gives rise to multinucleate spores—and a vast number of fungi do this—or even in "heterozygous" fungi with uninucleate spores, a single-spore culture may, after very little growth, have given rise to a mixed population, and that when this culture is exposed to selective influences it is entirely reasonable that certain strains in this population should be favoured to the exclusion of others. If segregation has been complete and selection is sufficiently stringent a single "new" strain, perhaps quite different from the parental culture, will be produced; and, if pure, will remain constant even when returned to the original conditions, for there will be no segregation and no subordinate strain to be reselected. If segregation has been incomplete or selection insufficiently stringent, then, on the return of the culture to the original conditions, the original parental form may again become dominant. Let me give only one illustration. Of certain strains of *Rhizoctonia solani* Matsumoto[24] writes: "Fusion is also observed . . . between P1 and P4." "In no case may the strain P4 be so changed as to resemble the form P1, either by changing the culture media or host plants, while P1 may be easily transformed into P4." Now if this be mutation or evolutionary change, we have to assume as its basis some very strange hereditary mechanisms. But is it not surely more simple to assume that in the hyphal fusions, nuclei (the cells of Rhizoctonia contain many nuclei) from the homocaryotic line P4 have passed into the cells of P1, and that subsequent growth of the latter has produced genetically impure mycelia? The strain P4 is genetically pure and therefore constant. The strain P1 is genetically impure, and under certain conditions nuclear segregation may occur, and reproductive moieties containing only nuclei of P4 are cut off. These, being favoured by the conditions, rapidly become the dominant strain to the exclusion in the culture of the original strain P1. The moment one has grasped the fact that a single-spore fungal strain may be heterocaryotic, and one has realized that the formation of septa by the ingrowing of a diaphragm unrelated to nuclear division or to the number or condition of the nuclei in a multinucleate fungal cell may, if conditions are favourable, give rise to nuclear segregation, one has the key to "variation" and "mutation" in the fungi, and all the phenomena fall into line.

It must be understood that I do not deny the possibility of mutation, even in the narrow sense, in the fungi (to deny the possibility of anything is unscientific); but it is my considered opinion that no single case has yet been described which satisfies the criteria essential to the proof of mutation; nor, until we know vastly more about the genetic constitution and possibilities of individual fungi, can we hope to satisfy such criteria. Personally I find it much more in keeping with the data of mycology as they present themselves to me to interpret all described cases in the manner I have indicated. This applies even to the instance I have myself put on record.[7]

BACTERIA.

There is a vast amount of bacteriological literature bearing on our subject and references to the more recent investigations may be found in the files of *Bacteriological Abstracts*. The older literature has been collected together and discussed in the works of Adami,[1] Gurney-Dixon,[11] Löhnis,[21] and others.

Before entering into any detail, attention may be drawn to certain general issues which are usually overlooked. It has been noted that the genetical concept of mutation is based on the premiss of certain types of change in one portion or another of the hereditary mechanism, and that to ascertain whether any apparently hereditary change is a mutation or not we must be able at least to analyse the genetic values of the parent and the "mutant," and confirm this factorial analysis, if possible, by cytological data. Unless we can do

this we are very much in the position of a chemist trying to prove mutation of the elements without being able to analyse chemically his initial or his end materials.

Now, quite briefly, we do not know the genetic constitution of a single micrococcus, nor, in the present state of our technique and knowledge, can we ascertain it; and we have no knowledge whatever of the structure or type of hereditary mechanism in bacteria. That bacterial strains may remain constant for years under the most varied conditions, and passing through a myriad generations, implies an extremely delicate and constant hereditary mechanism, quite comparable in its behaviouristic fixity and nicety to that in higher organisms. Of this mechanism we know nothing, and still discuss whether or not bacteria contain nuclei at all. Until, however, we can evaluate the genetic constitution of bacteria, and until we can learn something of the hereditary mechanism involved, to use the term "mutation" in bacteriological studies, and to equate the concepts in genetical studies of higher organisms with those of bacteriology, is mere obscurantism. The only changes of which there is evidence in bacteria are modifications and apparently hereditary changes of unknown genetic causation.

A further point to be noted is that whereas in genetical studies of higher organisms it is essential that the experimental subject be the *individual* of known genealogy, in bacterial studies the experimental subject has been the *culture* containing myriads of individuals whose genealogical relationships are entirely unknown, save that the total culture may be derived from one or more ancestral cultures, or in the rarest cases from a single individual progenitor. The unit in bacterial researches corresponds not with the *individual* of the geneticist, but with the *formation* of the ecologist, and genealogical relationships in bacteriology are from ecological formation to ecological formation, and not from individual to individual. Yet, in bacteria, as in all higher forms, hereditary changes do not concern ecological units, but have immediate causation and expression within the individual *qua* individual.

Again, a fact of immense importance is that not only do we know extremely little concerning the structure of single bacteria, but that we know even less of the developmental and reproductory phases in the life cycle of individual bacteria. Löhnis[21] and Hort[15] have been the foremost among the few students who have tried to remedy this defect; and although there is much in the recent volume of Löhnis with which one may not agree, it is undoubtedly the most courageous and the most valuable contribution to this aspect of the subject yet published, and a work demanding the closest attention from all bacteriologists. That bacteria may develop through cyclical phases of far greater complexity than the simple life history still taught in textbooks and naïvely believed by many workers, surely stands in no further need of substantiation. In fact, until we know vastly more about the physiological and morphological phases of development and the reproductory processes in the life cycle of *individual* bacteria it is altogether premature to speak of hereditary changes in bacterial type. In the face of this ignorance the assumption that "it seems quite justifiable to consider bacteria as asexual throughout their life history"[2] which underlies all researches on bacterial variability is a perfectly gratuitous one. All that can be said is that sexuality has not yet been demonstrated, and considering the difficulties attendant upon such work, and the fact that practically no attention has been given to its demonstration, this is not surprising. A comparative view of the results of bacterial research points straight to the occurrence of some form of genetic impurity in bacterial individuals. If it be not so then the bacteria are a class unique unto themselves.

It will be evident, I think, from what I have said that we are ignorant of nearly all the facts and criteria which are essential to any critical study of bacterial genetics. The literature on bacterial variability is chaotic and full of incompatible and contradictory statements and descriptions of phenomena; but from a broad view the principal issues relating to our problem which seem to emerge may, perhaps, be outlined as follows:

(*a*) There are a great number of bacterial strains which differ from each other to a greater or lesser degree and often live commensally.

(*b*) Certain strains may remain constant under either unvarying or changing conditions, whereas certain cultures of other strains may show no variation, whilst parallel cultures of these strains may show variation to a greater or lesser degree.

(*c*) The change may apparently take place gradually or suddenly; it may be an apparently permanent change, or it may be that selection appears to be effective in either or both *plus* or *minus* directions; it may be a fortuitous change, or it may apparently be a specific and teleological response to one or more particular operative stimuli.

(*d*) No experimental research has been carried out by the following of single individuals of known origin through many generations (Hort has come the nearest to this), and only in the rarest cases is any experimental culture to be referred back to a single individual progenitor (and I very fully agree with Hort's adverse criticisms of the isolation methods in vogue!).

When viewing broadly the chaos of bacterial literature relative to variation I do not think anyone knowing the literature of "genetics" and evolutionary thought prior to about the beginning of the twentieth century can fail to be impressed with their essential similitudes. Identical phenomena are described, similar points of view are adopted, and the same interpretations appear. Genetical students of higher forms relinquished these ideas between the years 1900 and 1910. About the latter date protozoologists began to reach out to a more exact *pure-line* formulation of their problems. Mycologists are just beginning to see the light, but with rare exceptions bacteriologists are still in the darkness of a pre-Mendelian and a pre-Johannsen period.[18,19]

If one considers the more theoretical aspects of the published data on bacterial variability there would appear to be only two feasible interpretations:

(*a*) It may be assumed that the bacteria possess an hereditary mechanism sufficiently delicate to distinguish, for example, between different blood serums or temperatures only separated by one degree, and yet sufficiently static to do this without appreciable variance for innumerable generations; a mechanism which, according to the results of reliable students, must be almost uncannily intelligent, overcoming the specificity of enzymes, to adapt itself to new conditions in a manner quite unequalled by higher organisms, and yet a mechanism which, according to reliable students, must be of the most labile order, in certain cases being apparently at the mercy of almost any chance condition and changeable at will with the greatest ease. As I have indicated, the bacteria would then be a unique class of organisms and any deductions drawn from their behaviour would be quite inapplicable to any other organisms.

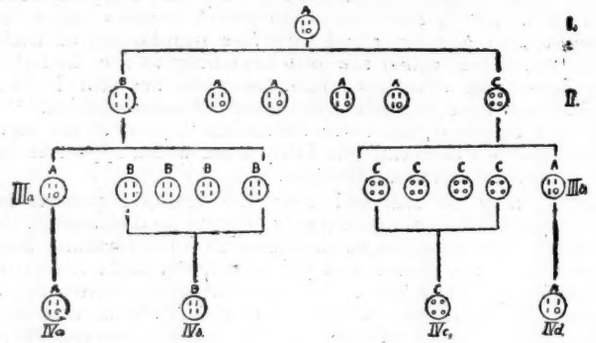
(*b*) On the other hand, we may assume that bacteria possess an hereditary mechanism comparable in all behaviouristic respects (we know nothing about structural aspects) with that of other organisms, and the occurrence, perhaps only at rare intervals, in the bacterial life cycle of some form of cell fusion giving rise to genetic impurity.

Both hypotheses would fit the known data on the behaviour of bacteria. Now without entering into any detail, but bearing in mind our ignorance of the genetical constitutions, hereditary mechanisms, and life cycles of the bacteria, and the fact that the data have been derived from researches in which the unit basis has been the ecological formation and not the individual organism, the second of the above alternatives would seem, to my way of thinking, overwhelmingly the more reasonable. Granting the assumption of the occurrence in bacteria of genetic impurity with some mechanism of segregation, all the data concerning variability and apparent mutation in this group fall astonishingly well into line with modern genetical conceptions. Genetically impure single bacteria might occasionally or frequently give rise to mixed populations, just as genetically impure fungus spores might do, or just as single plants of, for example, *Oenothera lamarckiana* would do, were all their seed and that of their offspring sown for a myriad cross-breeding generations. As has been described for the fungi, selective influences might then change the whole physiological or morphological facies of a bacterium in culture merely by the favouring or isolation of its constituent strains. According to the degree of segregation and the stringency of selection the change would be temporary and susceptible to reverse selection, or permanent.

Perhaps one may be forgiven for showing the manner of this change diagrammatically and reduced to its simplest proportions; but it must be realized that in actuality such changes are probably of an extremely complex order.[6]

A genetically impure bacterium, A, would, in the vast majority of its progeny, merely reproduce its own condition, A,

but occasionally it might segregate out its constituents in pure form, B and C. These latter would be present in such infinitesimal proportion as to be totally inappreciable in the culture. If, however, the conditions were so changed as to favour the development of the submerged forms, these might gradually become the dominant forms, as in III*a* and III*b*. If the selection were sufficiently stringent over a large enough number of generations the submerged form A might die out, leaving pure cultures of either B or C, IV*b* and IV*c*, according to which of these types had been favoured. An apparently new form would now have been developed, which, as it is genetically pure, will remain constant. If, however, the original impure type A has not been completely eliminated it may again gradually become dominant on the reversal of the selective processes. The longer a genetically impure strain is subjected to the action of selective conditions the purer genetically it will become, and the longer it will take to "revert" when returned to the original conditions. This phenomenon, so simple on a basis of genetic impurity in the *individual* bacterium, has been a most fruitful source of controversy and speculation in the past,[1] [11] and is now being found of "extreme interest" by protozoologists.[17] [20]



In this consideration I have not entered into detailed criticism of the several experimental results, but have kept the treatment broad and comparative, and tried to indicate the relative value of certain genetical concepts in the biological groups that concern us. If our studies are to be of value it is imperative that we have our foundations firm and straight in the solid knowledge of structure and life history of individuals before we investigate variations in superstructure and make positive claims regarding mutation. We must have our criteria precise and cleanly cut.

SUMMARY.

A mutation is not any old change in an organism. In the strict sense it is a change in the fundamental nature of one or more hereditary units; in the wider sense it is a change in genotype due to abnormal distribution of hereditary units. A change originating in normal distribution of hereditary units in crossing, or as a result of redistribution due to the appearance of any form of genetic impurity in the parent individual, is not a mutation.

The proof required to establish biological mutation is no less precise than that required to establish chemical mutation; and like the latter is dependent upon analysis of the original and the derived products.

The concept of mutation held at present by most microbiologists cannot be equated with that held by genetical students of higher forms. It is a more nebulous and inclusive concept, for their subjects have not yet been found susceptible to factorial analysis by cross-breeding, and detailed cytological information regarding the genetic structure and behaviour of the hereditary mechanisms is not available.

In the protozoa and fungi there is the possibility of the origin of apparently new and distinct strains in the normal cytological processes, and it is suggested that apparent mutations may be due to the selective isolation of such strains.

In the bacteria it is suggested that a similar hypothesis is the most feasible on which to interpret present data; and that it will explain the facts of "variation" and "mutation," bringing them into line with genetical concepts.

The possibility of mutation in micro-organisms is not denied; but it is considered that no adequate proof of such mutation has yet been adduced, or can be in the present state of our technique and knowledge. We are trying to write before we have learned our alphabet!

REFERENCES.

[1] Adami, J. G.: *Medical Contributions to the Study of Evolution*. London, 1918.
[2] Arkwright, J. H.: Variation in Bacteria in Relation to Agglutination both by Salts and by Specific Serum. *Journ. of Path. and Bact.*, 24, 1921.
[3] Banta, A. M.: *Selection in Cladocera on the Basis of a Physiological Character*. Carnegie Inst., Washington, No. 305, 1921.
[4] Blakeslee, A. F.: Mutations in Mucors. *Journ. Heredity*, 11, 1921.
[5] Bridges, C. B.: Genetical and Cytological Proof of Non-disjunction of the Fourth Chromosome of *Drosophila melanogaster*. *Proc. Nat. Acad. Sci.*, 7, 1921.
[6] Brierley, Wm. B.: Some Concepts in Mycology. *Trans. Brit. Myc. Soc.*, 6, 1919.
[7] Brierley, Wm. B.: On a Form of *Botrytis cinerea* with Colourless Sclerotia. *Phil. Trans. Roy. Soc.*, Ser. B, 210, 1920.
[8] Burgeff, H.: *Untersuchungen über variabilität, sexualität und erblichkeit bei Phycomyces nitens, Kunze*. Flora 7, 1914.
[9] Burger, O. F.: Variations in *Colletotrichum gloeosporioides*. *Journ. Agric. Res.*, 20, 1921.
[10] Erdmann, R.: Endomixis and Size Variations in Pure-bred Lines of *Paramecium aurelia*. *Arch. f. Entw mech.*, 46, 1920.
[11] Gurney-Dixon, S.: *The Transmutation of Bacteria*. Cambridge, 1919.
[12] Haeincke, A.: Vererbungsphysiologische untersuchungen an arten von Penicillium und Aspergillus. *Zeit. f Bot.*, 8, 1916.
[13] Hegner, R. W.: Heredity, Variation, and the Appearance of Diversities during the Vegetative Reproduction of *Arcella dentata*. *Genetics*, 4, 1919.
[14] Hegner, R. W.: The Relations between Nuclear Number, Chromatin Mass, Cytoplasmic Mass, and Shell Characters in Four Species of the Genus Arcella. *Journ. Exp. Zool.*, 39, 1920.
[15] Hort, E. C.: The Reproduction of Aerobic Bacteria. *Journ. Hygiene*, 18, 1920.
[16] Jennings, H. S.: *Life, Death, Heredity, and Evolution in Unicellular Organisms*, 1920.
[17] Jennings, H. S.: Variation in Uniparental Reproduction. *Amer. Nat.*, 56, 1922.
[18] Johannsen, W.: *Uber erblichkeit in populationen und in reinen linien*. Jena, 1903.
[19] Johannsen, W.: *Elemente der exakten erblichkeitslehre*. Jena, 1909.
[20] Jollos, V.: Experimental protistenstudien. *Arch. f. Protistenkunde*, 43, 1921.
[21] Löhnis, F.: Studies upon the Life Cycles of the Bacteria. Part I. *Mem. Nat. Acad. Sci.*, 16, 1921.
[22] Lotsy, J. P.: *Evolution by Means of Hybridization*. Nijhoff, 1916.
[23] MacCurdy, H. M.: Division, Nuclear Reorganization and Conjugation in *Arcella vu garis*. Mich. Acad. Sci. Ann. Repts., 21, 1919.
[24] Matsumoto, T.: Physiological Specialization in *Rhizoctonia solani* Kuhn. *Ann. Mo. Bot. Gard.*, 8, 1921.
[25] Mavor, J. W.: On the Elimination of the *X*-chromosome from the Egg of *Drosophila melanogaster* by *X* Rays. *Science*, N.S. 54, 1921.
[26] Morgan, T. H.: *The Physical Basis of Heredity*. Philadelphia, 1919.
[27] Muller, H. J.: Variations due to Change in the Individual Gene. *Amer. Nat.*, 56, 1922.
[28] Orban, G.: Untersuchungen über die sexualität von *Phycomyces nitens*. *Beitr. Bot. Centr.*, 36, 1919.
[29] Schiemann, E.: Mutationen bei *Aspergillus niger* van Tieghem. *Zeit. f. Indukt. Abstam.*, 8, 1912.
[30] Schouten, S. L.: Väriabiliteit bei schimmels. Handel, 16e. Nederl. Natuur.-en Geneeskundig. Congr. Gravenhage, 16, 1918.
[31] Thom, C., and Church, M. B.: *Aspergillus flavus, A. oryzae* and Associated Species. *Amer. Journ. Bot.*, 2, 1921.
[32] Waterman, H. I.: Sur l'action de quelques dérivés du benzène sur le développement de *Penicillium glaucum*. Kon. Akad. Wetensch, Amsterdam, 15, 1912.

DISCUSSION.

Professor DAVID ELLIS (Royal Technical College, Glasgow) said: It is well known to bacteriologists that the adaptability of bacteria to changed conditions, whether internally or externally excited, is far greater than that possessed by organisms higher up in the scale of evolution. We could, therefore, on *a priori* grounds expect in the bacteria that capacity for change which has sometimes brought systematists to the verge of despair. Instances of rapid changes in form and function on the part of bacteria are numerous and appear in every aspect of bacterial life. On very slight provocation bacteria change in shape and size, their virulence and their growth in various media are subject to change, as also is their reaction to various stains and their power of fermenting sugars. It is necessary to remember that our test-tube cultures are very artifical, and that it is possible to produce relatively as much change in three months as would take place only after a hundred years or more in the breeding of some of the higher animals or plants. The most potent of such external changes is, in my experience, the influence of the mass of excreted matters which the bacteria liberate into the surrounding medium. For instance, when test-tube cultures of the Coccaceae were subcultured at frequent intervals most marked changes were observed. The cells lost their cohesiveness and became reduced from large masses of cells to tetracocci, tricocci, diplococci, and even unicocci. I found this procedure to take place in all of 19 different species of Sarcina that were investigated. The change was followed by a gradual development of motility. The final result showed organisms that were so different from the first cultures that they would certainly not, if considered casually, be placed in the same species or even the same genus. All had been brought about by the simple process of removing the cells at the earliest opportunity from their own excretions by frequent subculturing. Every coccus is, therefore, potentially capable

of movement and of existing apart from its fellows as a single cell.

Those functions in the higher plants for which the nucleus is responsible are carried out in the non-nucleate plants and animals by the general body of the cytoplasm. We may therefore assume that the germinal or hereditary characters are located in the lower non-nucleate organisms in ultramicroscopic particles in the cytoplasm. Such particles may be regarded as physical or chemical entities capable of autocatalysis, and as such are to be regarded as possessing a value equal to the potentialities that are lodged in the chromosomes of the higher organisms.

Under certain circumstances *Crenothrix polyspora*, one of the iron bacteria, departs from its normal activities, and lashes itself into a frenzy of reproductive activity. From single threads thousands of minute specks ooze out in zoogloeae. These specks do not cease to multiply after leaving the thread. Under ordinary circumstances *Crenothrix polyspora* is a placid organism, and pursues its path in life in a very leisurely fashion. If, however, a particular set of conditions arises it departs suddenly from its habits, and on occasions causes a large sheet of water within a short time to assume a very dirty appearance. The transformation has been effected from a comparatively large thread-like organism to one which is composed of coccus-like specks of living matter which, so far as one can judge, never return to the thread condition.

A somewhat dramatic change may take place in the life history of *Cladothrix dichotoma*, another of the iron bacteria, which lives in water in the form of long threads, each containing a single row of short rods, and which can be cultivated artificially without much trouble. Its method of reproduction is very simple, and consists solely of cell division of exactly the same nature as takes place when the bacillus divides vegetatively. The new rods that are thus formed escape usually from the top, sometimes laterally. It was stated by Zopf many years ago that sometimes spirilla issued laterally from the Cladothrix thread. His statement was received with incredulity by the majority of bacteriologists, but on reading over his papers I found Zopf's statements to bear such an impress of the truth that I resolved to repeat the experimont. I may state that for eighteen months I made almost weekly subcultures under open conditions of Cladothrix, and at last, after eighteen months, my patience was rewarded, and I observed the complete change that took place in Cladothrix, which followed the course described by Zopf. It was that spirilla issued forth from the threads instead of straight rods. These spirilla had polar cilia and were composed each of a few rods, but some were composed of one rod which assumed a spiral form and was propelled by polar cilia. Unfortunately, it was not possible to follow the further development of these spiral rods.

Among the iron bacteria are two very common forms in this country, which are named *Leptothrix ochracea* and *Gallionella ferruginea* respectively; there is a third in the same habitat which I found and named *Spirophyllum ferrugineum*. In my further investigations of these three organisms I found that, except for their outward shape, there was absolutely nothing to distinguish between them. They also practically always occur in association, usually two out of the three, rarely three together. I hold that these are all pleiomorphic phases of one and the same organism.

He must be a courageous investigator who sets out to prove mutation for bacteria in such a way as to satisfy the criteria imposed by the geneticists of the higher forms. He is debarred from making deductions from cytological considerations owing to the extreme minuteness of the forms and their rudimentary structure. He cannot undertake factorial analysis by cross-breeding, for no fusion of cells has been observed. It really comes to this, that proof of mutation is obtainable only in the case of cells of plants with a certain measure of differentiation and with sexual reproduction. And yet one feels that the phenomenon of mutation happens in bacterial life just as much as, or probably more so than, in higher organisms.

Dr. F. W. Twort said: I do not agree with some of the rules laid down by Dr. Brierley. It seems to me that there can be no such thing as genetic purity in biology, certainly nothing comparable to the chemical purity of an element. I also look upon differentiation of morphology as a very crude method of attempting to demonstrate mutation changes.

Dr. J. S. Dunkerly (Glasgow) said: While agreeing cordially with Dr. Brierley's remarks on the need for strict genetic criteria for distinguishing true mutations, it is possible that we find in protozoa one of the characteristics required by Dr. Brierley's tests—namely, a fundamental change in the nature of the chromosomes. In Arcella and other protozoa the chromidia, as distinct from the nucleus, can form secondary nuclei, which carry on the life cycle. Chromidia can even undergo "chromidiogamy," a method of syngamy in which the nuclei take no part whatever. This may produce a fundamental change in the chromosomes, inasmuch as they are formed anew from chromidia, not from the previously existing chromosomes of the nucleus.

Dr. J. C. G. Ledingham (Lister Institute, London) said the opener claimed that no adequate proof of mutation of bacteria had yet been adduced, nor could be adduced, in the present state of our technique and knowledge. If we accepted Dr. Brierley's definition of a mutation as a change in the gene of a homozygous individual, he was simply stating a truism, as the genetic constitution of bacteria could not be analysed on lines employed, for example, with plants and animals. He had described the literature of bacterial variation as chaotic, and doubtless it was so to one looking at the subject entirely from the genetic point of view; but Dr. Brierley should be aware that an extremely small proportion only of the vast mass of literature on this subject was the result of work directly instituted with a view to inquiry into possible genetic mechanisms. The great mass of the work on that subject had been carried out almost solely with the object of explaining peculiar behaviour and peculiar phenomena in bacterial growth and metabolism, and the contributions of bacteriologists to this field had undoubtedly enriched our science. The few contributions to the purely genetic side (if such existed in bacteria) by Hort, Löhnis, Bergstrand, Mellon, and others were regarded by most bacteriologists as still very much *sub judice*. In any case the evidence of the existence of cycles in bacterial life was as yet too small and flimsy to carry much weight on the general question. The terms "mutation" and "mutant" had doubtless often been used rather loosely by bacteriologists; but, as a rule, they had been taken to connote truly hereditary changes, temporary adaptation to environment being excluded. If mutation was used in the sense of an "hereditary change of unknown genetic constitution" as de Vries used it, it did not seem inappropriate in bacteriology, and until we could talk about genetic constitution in bacteria it seemed justifiable to employ the word "mutant" in this sense. In dealing with a group of organisms such as bacteria, which had not proved susceptible of analysis into organs or genetic elements, it seemed wiser to deal with the phenomena only and to steer clear of genetic hypotheses derived from analogy. Whether these phenomena were due to change in the germ substance of the organism or to suppression of parts of it as had often been suggested, or whether they were due simply to selection of individual constituents of the race, might not be established in all cases, but nevertheless these phenomona were of immense importance, though the exact name for them might be disputed. In his opinion, the single-cell procedure in bacterial work had not advanced matters very much and had taught us little more than the single colony. Finally, if all hereditary changes leading to evolution of species were held, as Dr. Brierley suggested, to be due to primary impurity of strain, the attempt to demonstrate the genetic mechanism of the change was, from the practical point of view, not worth making.

Dr. Brierley, in reply, said: The evidence that has been brought forward by Dr. Ellis regarding pleomorphy and cyclical changes in bacteria is of great interest and all goes to show our almost complete lack of knowledge of the basic facts of bacterial genetics. Much more work of this kind must be carried out before we shall be in a position to understand hereditary changes in bacterial type or to evaluate them in the concepts of the genetics of higher forms. I am, however, entirely opposed to Dr. Ellis when he would make genetical criteria more inexact in order that our vague and partial knowledge of the genetics of micro-organisms may be interpreted. Surely it is not our criteria that must be rendered nebulous and inexact but our knowledge of micro-organisms that must be made less nebulous and more exact. The standpoint of Dr. Ellis in this particular matter seems to me a negation of all scientific methodology.

Dr. Twort comments upon the fact that I insist upon a state of genetic purity as a *conditio sine qua non* in investigations having as their end the elucidation or artificial production of mutation, and asks me, "What is genetic purity?" This is no easy question to answer where micro-organisms are concerned. In higher organisms we can obtain a state of genetic purity or homozygousness by certain definite breeding techniques, a long story into which I cannot enter here. In micro-organisms the application of such techniques is not yet possible, nor have we any method of genetically analysing our organisms or of uncovering recessive factors. We have, therefore, no means of ascertaining whether or not micro-organisms are genetically pure, which is one of the reasons why I find it premature to use the concept of mutation. Dr. Twort also states that morphology is far too coarse a criterion for bacterial mutations, which are of a more subtle character. I fully accept this, but cannot quite see its bearing on our discussion, for physiological powers are the expression of germinal qualities under particular conditions just as are morphological structures.

Dr. Dunkerly has indicated very clearly some of the complexities to be met with when the hereditary processes of such a form as Arcella are considered. The point that I have tried to make is that these and similar complexities are always overlooked. It is just the presence of such little understood hereditary mechanisms that makes it premature to equate genetical concepts as between higher organisms and micro-organisms. So far as I understand Dr. Ledingham's criticism, it is that evolution can only have come about if mutation has occurred, that we accept the reality of the evolutionary process, and therefore must accept the phenomenon of mutation as a fact. Granted the premisses, this conclusion would be true; but must we accept the premisses? I think not, for there are surely many other modes of evolution than that of mutation. It is a very valuable thing to hear from Sir William Leishman that this evidence, on which much doubt has been cast, may be accepted. It is fundamental work and must go far to revolutionize our concepts in bacteriology.

AN OUTBREAK OF FOOD POISONING BY MILK, CAUSED BY B. AERTRYCKE.

BY

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OWING to the remarkable circumstance that British records contain only ten instances of epidemic food poisoning due to the distribution of cow's milk, and that in only three of these has the causative micro-organism been completely identified, it has been considered of importance to administrative authorities and of interest to bacteriologists to bring forward the facts contained in this communication. I am not concerned with these ten outbreaks except from the bacteriologist's point of view. Notice of one of them was brought before the Royal Academy of Medicine, Ireland, by Dr. O'Kelly in March, 1922.[1] It was due to the *B. enteritidis* Gaertner. The other nine are collated by Dr. W. G. Savage in his recent publication, *Food Poisoning and Food Infections* (1920).[2] Although in that list of nine the organism is given as completely fixed in three cases and as being *B. enteritidis* Gaertner, I have to note that in one of these three (the Chorley outbreak of 1914) the report of Professor Delépine, which is given in the county medical officer's report,[3] describes the causative organism repeatedly as a "bacillus of the *enteritidis* type." Notwithstanding that in the last few words of his report Delépine makes the statement that "the *Bacillus enteritidis* isolated from the ear of the cow was capable," etc, this report does not fix the organism bacteriologically as other than a member of the Gaertner group. I take the number on the list, therefore, as reduced to two. While, then, the specific organism has been fixed on three occasions as *B. enteritidis* Gaertner, the outbreak now under review brings to notice an organism which to our knowledge has not hitherto figured as the cause of an epidemic due to the consumption of cow's milk—namely, *B. aertrycke*.

THE PAISLEY OUTBREAK.

The details of this outbreak admit of quite a short description. On May 8th (a Sunday), 9th, and 10th, 1921, there occurred in Paisley a considerable number of cases of sudden illness characterized by headache, abdominal pain, vomiting, and diarrhoea, with temperatures, generally low, reaching in some cases to 101° to 102° F. They were investigated by the medical officer of health of the town, Dr. G. V. T. McMichael, and to his courtesy I am indebted for permission to make use of the epidemiological facts which were ascertained. Fifty-two people, whose ages ranged from 14 months to 83 years, were struck down by the outbreak, and these involved eighteen families. A thorough investigation of the various food supplies of the patients was immediately undertaken, and it soon became apparent that the only source of food supply common to them all was a particular dairy which retailed cow's milk. The affected area was very circumscribed and therefore easily handled. In addition to this exclusive fact of common supply the suspicion that fell upon the dairy was heavily supported by certain other facts that emerged. Among these were the following facts:

1. Three patients, a husband, wife, and child, whose ordinary milk supply was from a different source, were supplied by this dairy on Sunday, May 8th; two of them sickened within twenty-four hours and the third within sixty hours. These three people were lodgers in a house occupied by two tenants who did not deal with this dairy and who escaped illness.
2. Four of a certain family (which figures in the bacteriological investigations) receiving this milk were affected, while the fifth member of the family, a child of 5 years, who did not receive milk from this dairy, escaped. This family were ordinarily supplied from another source, but on May 7th and 8th were supplied by this dairy. Three members sickened on May 9th, and one on the 10th.
3. Of a certain family of ten (which also figures in the bacteriological investigations) eight suffered, one escaped, and one was breast-fed. This family received the same milk supply as the family of five just mentioned, while it was ascertained to a nicety that their other food supplies differed *in toto*.
4. There was still another family which was not served by their usual dealer on May 8th but by the dairy in question, and two of its members sickened the following day.
5. The age of some patients was as low as 14 months.

In addition, it has to be stated that five cases of illness where a different milk supply was used came under notice. Three of these were individual members of three households where the other members made no complaint, and two of them were brothers of a girl under treatment at the time for influenza. They are therefore not serious exceptions and do not invalidate the conclusion of the medical officer of health as to the defaulting milk, and they do not exceed in number the apparent exemptions encountered in the majority of such investigations.

In view of all the facts the opinion of the medical officer of health which incriminates the milk supply is conclusive. The mortality in the epidemic was *nil*. All the patients were convalescent in a week.

Source of the Contamination.

It has to be said at once, to the disappointment of administrative officers—a disappointment to which, unfortunately, they are not unused—that the source was not determined. The dairy in question had its supply from two farms, one of which was unexceptionable in every respect, while the other was just the reverse. This second farm had been well known to the medical officer of health of the Barrhead district as quite unsatisfactory from a hygienic point of view, and Dr. McMichael's inspection confirmed this opinion. At the same time this inspection, which was carried out in concert with the Paisley veterinary inspector, did not reveal any signs of disease or illness among any of the animals, and the farm people themselves professed to be well, and refused to submit themselves to examination by a bacteriologist. It was said to be a fact that part of the consignments reaching Paisley on these days (May 7th to 9th) was reserved for ice-cream dealers in Barrhead and delivered thereto, but that no case of similar illness occurred there. Though grave dissatisfaction attached to the conditions obtaining at this farm, its milk supply was not stopped, and no further case occurred after May 11th.

Bacteriology of the Outbreak.

General Remarks.—It was a rather awkward circumstance that the bacteriologist called upon resided in a different town. He would otherwise have been in contact with the materials sooner than he was—an all-important matter. As it was, the writer received two samples of milk on May 24th, fifteen days after the commencement of the outbreak, probably for the purpose of clearing the farm rather than for detecting the source of contamination. They revealed nothing. With such an elusive organism as *B. aertrycke* one could have no reasonable hope of finding it in these materials after such an

interval. The examination of the patients, however, was satisfactory, and established the cause of the trouble. Materials from four cases were submitted and three of these gave positive results. The incubation period was unknown in the majority of the cases, since their daily milk supply was uninterrupted close up to the time of illness. But there were cases supplied by this dairy only on this week-end. In one such family the latent period was twenty-four hours in two cases and seventy-two hours in one case. In another such family supplied only on May 8th, two members sickened on the 9th at 5.30 p.m. (hours of milk delivery not fixed). In a third family the utmost limit of the latent period in three cases is forty-eight hours, and may in fact be about twenty-four, though in one case it may possibly extend to seventy-two hours. The majority of the known cases show a latent period of twenty-four to forty-eight hours. It is no doubt determined by the amount of contamination ingested.

Character of the Stools.—One specimen, submitted on the fourth day of illness, from which *B. aertrycke* (Mutton type) was isolated, contained much mucus with traces of blood. It was not of a faecal character. A specimen from a second patient, giving a positive result and submitted on the seventeenth day of illness, was fluid and faecal in character, with no mucus. The importance of the character of the stools is great from the point of view of medical officers of health. For on their initial impressions as to whether or not the stools at the beginning of an outbreak are due to dysentery there may hang the very possibility of getting hold of samples of the actual food materials that were consumed.

Experimental Results.—The materials giving positive results were the faeces of two patients and the blood serum of a third; a fourth specimen was submitted on the seventeenth day and was negative. The agglutination reaction of this serum was diagnostic for the Gaertner group of bacilli. It reacted equally with a stock *B. aertrycke* and with the bacillus isolated from the faeces of one of the patients which was subsequently fixed as *B. aertrycke*. It failed to agglutinate *B. enteritidis* Gaertner, and reacted with *B. paratyphosus* B, Schottmüller, only at low dilution, 1 in 25. This was on the eighth day of illness, and during convalescence. The organisms from the two patients satisfied all the morphological and biochemical tests for bacilli of the true Gaertner group. They were highly pathogenic for guinea-pigs, and one of them, *B. "Smith,"* after twelve months' culture on agar, is still able to kill a 600-gram guinea-pig in six days after intraperitoneal injection of 0.1 c.cm. of a twenty-four hours' broth culture. The *post-mortem* appearance in this last test comprised emaciation, congestion of subcutaneous and intestinal veins, a patch of haemorrhage in the wall of the large bowel, and a spleen enlarged to thrice the normal. The bacillus was recovered from the spleen and heart blood. Since these bacilli were agglutinated in high dilution of a specific serum of *B. paratyphosus* B, and were negative to a *B. enteritidis* Gaertner serum, they were subjected for final differentiation to the absorption test.

The results of the tests show: (1) That one of the organisms, *B. "Smith,"* is identical with the Mutton type of *B. aertrycke*. (2) That the other organism, *B. "Todd,"* is *B. aertrycke*, but is of a different type from *B. "Smith,"* and while not identical with *B. aertrycke* ("N") has a close resemblance to it. It differs from the latter (*a*) in its uniform agglutinability, (*b*) in the fact that even after the heaviest absorptions, graduated up to eighteen slants to 1 c.cm. of diluted serum, it is impossible by means of it to absorb the secondary agglutinins for *B. "Smith"* and *B. aertrycke* (Mutton) from the *aertrycke* "N" serum. Only half the latter was taken out, although the primary agglutinins have gone.

A word of explanation is necessary as to what is meant by *B. aertrycke* "N." Towards the end of 1920 I received from the Lister Institute, on request, the two *aertrycke* types, Mutton and Newport. These were the cultures that were set in action in these experiments. When the experiments were well under way, the abnormal behaviour of this "Newport" culture became obvious, for in its agglutinability it differed widely from the Newport bacillus which figured in the work of others. In this communication it is therefore named "N." "N" is undoubtedly a member of the *B. paratyphosus* B food-poisoning group, but no time has yet been available for working out its exact relationship with *B. aertrycke* (Newport) No. 129, National Collection of Type Cultures.

For the purposes of this investigation the "N" bacillus has served well enough, since the primary requirement was a comparison with two known organisms. At the same time, the more normal behaviour of *B. aertrycke* (Newport) No. 129 suggests the possibility that *B. "Todd"* may be identical with it. At the time when the "N" bacillus was received by me the publication of the list of the National Collection of Type Cultures was not yet in existence. Whether or not the "N" bacillus is a new type or a modification of No. 129—I have not yet had the opportunity of determining this—it is interesting and unusual to find two distinct types of *B. aertrycke* figuring in the same epidemic.

Summary.

1. In May, 1921, an outbreak of food poisoning in Paisley struck down 52 persons of ages ranging from 14 months to 83 years, and has been shown to be due to the consumption of cow's milk. The actual source of the contamination was not discovered.

2. Regarding the administrative investigation of such outbreaks, although one of these has been known to occur in which the stools were of a non-dysenteric character, grounds have been adduced for the opinion that it is not wise to neglect the possibility of food poisoning in the presence of stools of a typically dysenteric character.

3. In this outbreak two distinct types of *B. aertrycke* were found, (1) the Mutton type, and (2) one closely approximating to the Newport type and probably identical with it. The control Newport type "N," of the absorption tests, was, as explained, doubtful.

4. Our records do not contain an instance of a similar outbreak due to cow's milk containing *B. aertrycke*.

References.

[1] Dr. O'Kelly, British Medical Journal, 1922, vol. i, p. 682. [2] W. G. Savage, *Food Poisoning and Food Infections*, 1920, pp. 76–80. [3] Special Report of the C.M.O.H. (Lancashire), 1911.

THE TOXINS OF B. DYSENTERIAE SHIGA.

BY

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A review of the extensive literature dealing with the toxic properties of the *B. dysenteriae* Shiga reveals the fact that various views are held as to the method in which the organism elaborates its toxins and produces its pathological effects. Earlier workers, including Todd, Doerr, Conradi, and Dopter, seem to favour the view that the soluble toxin of this bacillus is of the nature of an exotoxin. Recent research on the subject by Olitsky and Kligler indicates that two distinct toxins are elaborated by the organism—namely, an exotoxin and an endotoxin, which differ in their pathological action.

This endotoxin is said to be produced when the *B. dysenteriae* Shiga undergoes autolysis or dissolution of the bacterial cell. The exotoxin is said by these workers to be entirely a neurotoxin affecting the central nervous system, whilst the endotoxin is concerned in the production of dysenteric colitis.

Experimental Work.

In order to find out if the observations of Olitsky and Kligler would be confirmed on using the available laboratory strains of *B. dysenteriae* Shiga a well-known toxic strain was selected for the following experiments. The strain chosen was *B. dysenteriae* Shiga, Dean I, and for the sake of uniformity this strain was employed throughout the subsequent experimental work.

Experiments with Exotoxins.—Filtrates from alkaline broth cultures were prepared in the usual way, and 1 c.cm. and 2 c.cm. were injected both intravenously and subcutaneously into rabbits. These animals died within four days, showing symptoms of paralysis but no diarrhoea.

Experiments with Products of Autolysis.—Twenty-four hours' growth from Roux bottles was washed off with saline and the suspensions incubated for two days prior to filtration. The filtrate was heated to 80° C. for one hour. The series of animals inoculated with this filtrate also died, with one exception, within four days. No marked diarrhoea was observed prior to death, but the *post-mortem* examinations revealed extensive intestinal congestion, affecting principally the caecum and ascending colon. The rabbit which survived was subsequently inoculated with filtrate, which had only been subjected to 70° C. of heat for ten minutes. This animal died within twenty-four hours, showing well-marked diarrhoea, also paralytic symptoms. In order to arrive at an accurate means of estimating the minimal lethal dose of the killed strain of Shiga bacilli used

in subsequent experiments it was considered advisable to adopt weight in fractions of a milligram as a basis comparison. The opacity standards and counting chamber standards are liable to a large margin of error when minute amounts have to be considered.

The method adopted can be briefly stated as follows: A thick bacterial emulsion was prepared from Roux bottle cultivations. This emulsion was made with sterile water and subsequently dried by being exposed *in vacuo* on Petri capsule lids in a desiccator. The dry mass was then finely powdered in an agate mortar, any remaining moisture being driven off by exposure in a dry oven to 50° C. This powder was then stored in sealed glass tubes for further use.

The M.L.D. for a rabbit of approximately 1,000 grams weight was next determined. It was found that 0.01 of a milligram, equivalent to about twenty million bacilli, was just sufficient to kill a rabbit weighing 1,000 grams in three days. This quantity was accordingly counted as the M.L.D., though it was found later that rabbits vary very considerably in their resistance and that occasionally they could survive a slightly larger dose.

It was never found, however, that even a large rabbit could resist more than 0.05 of a milligram of dried Shiga bacilli injected intravenously unless it had been immunized by previous inoculation. Similarly the M.L.D. in the case of subcutaneous inoculation was found to be 1 milligram. In these experiments it was found that the rabbits died with paralytic symptoms, and practically no diarrhoea, and no *post-mortem* lesions of the intestines could be demonstrated beyond a slight congestion. Intestinal lesions did occur in cases of delayed death following partial immunization.

The extreme toxicity of *B. dysenteriae* Shiga has been an important factor in the causes of failure in using this organism in prophylactic inoculation for the prevention of epidemic bacillary dysentery. Very little success has attended any of the measures employed. The failure to produce immunity may be in part due to the fact that bacillary dysentery is not as a rule associated with a general blood infection, as is the case in the enteric group of diseases, but the chief difficulty seems to be in the impossibility of inoculating sufficiently large doses of vaccine owing to the severity of the reactions produced. If any method can be devised which will diminish the toxic properties of the bacillus without affecting its antigenic value, a certain advance will have been made in vaccine prophylaxis.

In 1916 Dean and Adamson found that eusol reduced the toxicity of Shiga bacilli; and more recently the results of Thomson in the detoxication of various organisms by treatment with alkali, etc., suggest a similar procedure with regard to *B. dysenteriae* Shiga. Very little work seems to have been carried out along these lines in the case of dysentery bacilli, although they form one of the most important groups for which a non-toxic antigen is required.

In association with Major Coppinger, R.A.M.C., I have found that it is possible to reduce the toxicity of Shiga bacilli by chemical means without seriously reducing their antigenic properties. The method employed does not involve the complete breaking down and solution of the bacilli as in Thomson's detoxicated vaccines.

The method employed is briefly as follows: To a thick emulsion of Shiga bacilli an equal bulk of liquor ammoniae fortis (*B.P.*) was added. The mixture was allowed to stand in the incubator at 37° C. overnight, and neutralization was then effected by means of 20 per cent. sulphuric acid, which, combining with the ammonia, formed ammonium sulphate. No precipitation occurred until the neutral point was reached, when a heavy precipitate immediately formed, which rapidly subsided to the bottom of the flask, leaving a clear supernatant fluid. On separating and washing the precipitate it was found to be composed of bacilli which were somewhat swollen and altered in shape, but still quite discrete and distinct. Inoculation into rabbits showed that these altered bacilli were relatively non-toxic, but retained their antigenic properties.

A series of experiments was now commenced with a view to testing the antigenic power of the altered bacilli and determining the most suitable doses for immunizing purposes. The immunizing effects were estimated by injecting toxic Shiga bacilli into rabbits previously inoculated with the new antigen, and judging from the results the degree of protection which had been conferred. In the absence of symptoms the condition of the rabbits was judged by the weight, an accurate daily chart of which was kept in every case. A series of ten rabbits was inoculated, five subcutaneously and five intravenously, with ammonia-treated bacilli. All the rabbits inoculated subcutaneously survived and suffered from no obvious reactions, either local or general, though in each case a distinct drop in weight occurred for a few days after the lethal test dose, which was subsequently injected intravenously. Three of the series injected intravenously died after the first inoculation, which was apparently too large. But the two which had received 0.1 mg. survived, and afterwards received larger doses without showing any ill effects. Moreover, the protection conferred was greater than in the case of the subcutaneously injected rabbits, as was judged by the fact that the injection of the lethal test dose produced no loss in weight. An attempt was now made to test the antitoxic effect of serum obtained from the seven rabbits surviving from the above experiments. Serum obtained from each of the rabbits was pooled and 1 c.cm. of the pooled serum injected into each of three normal rabbits along with 0.8, 0.4, and 0.1 mg. of unaltered Shiga bacilli respectively. The first two rabbits died, but the animal which had received 0.1 mg. (10 M.L.D.) survived, showing that the serum had acquired some degree of antitoxic power.

Human experiments on any large scale have not yet been carried out, but one of us received injections of 0.03, 0.075, 0.15, and 0.3 mg. of treated Shiga bacilli (approximately equal to 60, 150, 300, and 600 million) without any reaction beyond a slight local tenderness and induration, which, in the case of the largest dose, persisted some days.

Summary.

1. The observations of Olitsky and Kligler have been in the main confirmed, although it has been found almost impossible to produce "endotoxin" without at the same time having present a considerable quantity of the neurotoxin or "exotoxin."

2. Although two definite toxins for animals may be produced by Shiga bacilli, there is considerable evidence that the neurotoxin or "exotoxin" has a secondary action on the intestine through the initial inhibition of peristalsis prior to the passing of mucus or desquamated epithelial débris. The animal usually dies before the effect on the intestine becomes characteristic.

3. The oral administration of living Shiga bacilli to rabbits does not produce dysentery or any demonstrable serological reaction.

4. The toxicity of the bacterial substance of dried Shiga bacilli is not lessened by the treatment with acetone or the subjection to 100° C. for twenty minutes.

5. The various means of producing a detoxicated antigen from the bacterial substance appear to depend on the principle of macerating the bacillus with a strong alkali, then to neutralize the solution, and to utilize the resultant precipitate of less toxic bacterial substance.

6. The detoxication is only relative, but it affords a means of producing a vaccine which is useful in commencing the active immunization of animals. The intradermal reaction with this vaccine is not severe and large subcutaneous doses can be given without local reaction.

In conclusion, I beg to acknowledge my indebtedness to Sir William Leishman and to the professor of pathology at the Royal Army Medical College for assistance and encouragement in carrying out this experimental work.

BIBLIOGRAPHY.

Doerr: *Das Dysenterie Toxin*, Jena, 1907.
Boenche and Elkeles: *Zeit. f. med. Beamte*, Berlin, 1918, xxxi, 209.
Hoffmann: *Deut. Mil. Arztl.*, Berlin, 1918.
Olitsky and Kligler: *Journ. Amer. Med. Assoc.*, December 28th, 1918, p. 2125; *Journ. Exper. Med.*, January 1st, 1920.
Zotten: *Zeit. f. Immunitäts.*, 1920, 29.
Hiss and Zinsser: *Textbook of Bacteriology.*

THE DEVELOPMENT OF LOA LOA IN CHRYSOPS.

BY

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AND

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[*Abstract.*]

THE adult *Loa loa* lives in the connective tissues of man. The disease known as Calabar swelling is held to be due to the presence of this worm. In the course of its wanderings it occasionally crosses the eye, where it can be seen making its way under the conjunctivae. The female is longer than the male (about 2½ inches long), and is viviparous. The young make their way into the blood stream and are to be found there in greatest numbers during the daytime, hence their name, *Microfilaria diurna*. These embryos cannot develop further unless they find their way into another host. That host has been proved to be one of the Tabanid family of blood-sucking

flies, Chrysops. The geographical distribution of *Loa loa* and of Calabar swelling is limited to West Africa, and in certain areas of Nigeria the incidence is high. The investigations now to be shortly described were carried out in Nigeria during 1921.

Large numbers of Chrysops were allowed to feed on human cases in whose blood the embryos were present. Each fly was allowed to gorge itself on the patient. It was found that a rapid and uniform development of the embryos took place in all the flies (over 300) which were examined. The complete metamorphosis occupies a period of ten to twelve days. As ten days is the usual length of the cycle, it will be convenient to describe the changes from the first to the tenth day. Within an hour or two of passing into the stomach of the fly along with the ingested blood the embryos show active movements, which eventually enable them to cast their sheath. After a short period of comparative quiescence they penetrate the stomach wall of the insect and make their way to its muscular and connective tissues.

By the end of the first day most of the embryos are to be found near the tip of the abdomen, but they also occur in the thorax and in the head. The worm is now somewhat shorter than the original embryo, but thicker in its posterior third and with a finely drawn out curved tail. During the second day considerable thickening occurs and also an increase in length. Two vacuoles appear, one anteriorly and the other posteriorly. During the third day the worms grow still longer and thicker, and an alimentary tract develops. The alimentary canal is a slightly sinuous tube continued from the simple mouth at the anterior extremity backwards to the posterior vacuoles, where it ends in an anus. During the fourth and fifth days there is a greater increase in length than on the previous days and a lesser increase in breadth. On the sixth day the worm again casts its outer covering, and emerges with a trilobed tail, whilst the vacuolated areas have disappeared. On the seventh day a wide range of movement appears to be possible, and individuals may be found in the legs and in the roots of the wings of the insect. Up till the tenth day rapid lengthening occurs, with little or no increase in breadth. By the end of this period the worms are to be found congregated mostly in the head, at the root of the proboscis, and down into the labium. When next the Chrysops proceed to suck blood the young filariae swarm down through the labium and labella of the fly and pass on to the skin. Within a very few seconds they make their way through the pores into the tissues, where they proceed to develop into the adult form.

A full account of the foregoing investigations, along with references, will be found in the *Transactions* of the Royal Society of Tropical Medicine and Hygiene, 1922.

THE INTIMATE STRUCTURE OF THE BACTERIAL CELL.

BY

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IN spite of the extreme minuteness of the bacterial cell it is still possible to obtain a fair amount of information regarding its intimate structure. It must be confessed that this information cannot all be derived from a single species, and that the final picture is built up from material supplied from several sources. In order to get the best results the highest powers of the microscope must be directed on properly stained material by the light of a white cloud in the middle of the day during the summer season. The staining of the cell is very simple, but it must be carried out very carefully, and it must be borne in mind that the end in view is not necessarily the staining of the cell but rather the differentiation of its parts. The various constituents do not take up stains at equal rates, so that it is possible to obtain a differentiation of parts, which, however, persists only for a short time. With the continued penetration of the dye all parts become more or less uniformly stained and the differentiation is lost. The best results are obtained by the use of iodine solution, this fluid being allowed to penetrate slowly from the sides of the cover-slip. It is helpful to bear in mind that the bacteria are either generally regarded as primitive or degenerate members of the plants classed under fungi, and that we may thus expect that the general structure of the intimate parts of the bacterial cell will resemble that of the cells of this group.

It is generally agreed that the lower bacteria consist of three distinct groups: the Bacteriaceae, including the rod forms; Coccaceae, including the spherical forms; and Spirillaceae, including the spiral forms. It may be stated at the outset that the Bacteriaceae and Coccaceae form a natural group with very close affinities among its members, but that the group Spirillaceae is evidently widely divergent in its affinities. This fact is borne out in a most marked manner by the study of the intimate structure of the cells. Lastly, we have to take into account the intimate structure of the cells which make up the various thread bacteria.

The structure of the bacillus, the most typical of the Bacteriaceae, will be considered first. Its plant nature, and in particular its affinity to the fungi, is exhibited at every point. The cell is delimited on the outside by a sharply defined membrane which is shown very clearly by staining with any one of the usual dyes. It can be separated from the body of the cell by plasmolysis. It is elastic, as it is easily stretched and as easily returns to its former shape when the cause of the stretching has been removed. This, of course, is an obvious necessity in the membrane of a motile rod. It offers very little resistance to the escape of solid enclosed bodies to the outside. This can be observed when the application of a stain causes a greater swelling of the body of the cell than of the membrane. Bodies immersed in the former are sometimes pressed against the membrane and make their way out. Further consideration of the membrane will be postponed until we have dealt with the other cell structures. The body of the cell is made up of cytoplasm, which, however, is not homogeneous. By careful staining it is possible to make out a vacuolar network, such as is discernible in the cells of higher plants. In the vacuoles are doubtless located the same class of substances which are found in the vacuoles of higher plants—namely, raw materials, reserve materials, and other metabolic products, the formation of which is inevitably associated with plant or animal metabolism. In the cytoplasm of the bacillus the finer granules, plastids, etc., which have been observed in higher plants have not been discovered. We do, however, find solid particles of these reserve materials that are accumulated in quantity in the cell. These are *volutin*, the nitrogenous reserve material; *glycogen*, the carbohydrate form; and *oil globules*. These are the three main reserve materials in bacterial cells and are sometimes massed in fairly large quantities.

The question of the presence or not of a specific nuclear body is still a somewhat vexed one. It is certain that we are not likely to be able to settle the question by identifying the intimate structure of the nucleus in the bacillus with that of the nucleus of higher plants. It is, of course, not certain that if present the nucleus of the bacillus would conform to the highly complex nucleus which characterizes the cells of the higher plants. So far as can be judged from the information obtained by direct observation of the cell, all that can be stated is that in the cells one or more round bodies have been discovered which are different from the other bodies known to be present, and which take up the same stains as do the nuclei of higher plants. They are also present in the same relative proportion as are the nuclei of fungal cells. It comes to this, that the acceptance of the nucleus in bacterial cells is largely a matter of temperament. The strongest ground for acceptance appears to me to lie in the fact, which I now propose to discuss, that there appears to be a nucleus in control of the process which culminates in the formation of an endospore. As is well known, bacteria reproduce asexually by the formation of spores inside the cell. This process is common among the Bacteriaceae, so far as is known, is confined to one species among the Coccaceae, and is unknown among the Spirillaceae. In all cases spore formation is heralded by the appearance in the cytoplasm of a round or oval area which equals in volume the space occupied by the completed spore. This clear space is easily discernible by careful staining, with the end of intensifying the opacity of the cytoplasm, which in consequence makes the clear area more visible. The next stage in the development of the spore is a gradual absorption by something inside the clear space of material from the surrounding cytoplasm, with the result that the next stage in the development shows that what in the first stage was a clear space is now more opaque than the surrounding cytoplasm; and further, that the latter stains far less deeply than was the case in the first stage. It is clear, therefore, that the spore area once formed is capable of absorbing material unto itself, just as a solution of sugar contained in a permeable bag and

surrounded by water is capable of absorbing the latter substance until a certain pressure equilibrium has been reached. When the spore area is first formed it doubtless contains osmotically active substances which cause the absorption of certain ingredients which are necessary for the production of a spore. It is also obvious that the absorption is effected under the regulating influence of living matter, inasmuch as the completed spore contains living matter, and at no stage in the development of the spore is provision made for the introduction of protoplasm. Such being the case, the protoplasm must be present in the clear area when this is differentiated from the surrounding cytoplasm. The investigation into the spore development in the bacillus has shown that there is in the centre of the spore area a round substance, and this round substance has six or seven points on its periphery which suggest the remains of radiating strands from the round substance to the periphery of the spore area. The complete strands have not yet been observed, which is not surprising in view of the extreme minuteness of the organs under investigation. I may state that the same features have been observed in the only species among the Coccaceae which forms spores—namely, *Sarcina urinae*.

If we accept what the facts just mentioned suggest, then the spore area is a cell, the round object in its middle is a nucleus, and the points which beset the round object are the remains of cytoplasmic threads. In the third stage the effects of the osmotic activity are visible, inasmuch as the young spore—as we may now term the spore area and its contents—becomes deeply stained, whilst the rest of the cell is only extremely faintly coloured on the application of the appropriate dye. The spore has absorbed practically the whole of the contents of the cell. In a still further stage the spore is seen to be lying in an empty shell. By this time it has become surrounded by a well-defined membrane, and further, the membrane itself is made up of two layers, an inner thin one and an outer thicker one, which is often sculptured, in some species in a manner which can be used for the identification of the species. It is to the presence of the spore membrane that the resistant nature of the spore is due. The shell in which the spore is enclosed is the membrane of the cell in which the spore was formed, and if any doubt remained as to the presence of a specific membrane in the cell a glance at a spore lying in a bacterium from which everything had been abstracted except the membrane would settle the point. It is very curious to observe one of these spore-containing cells in a state of motility when it is practically empty of everything except the spore. There must, of course, in such cases be a fragment of cytoplasm still remaining in the cell, and it is to such a fragment that the flagella, the undulations of which have caused the motion are attached. In course of time the cell membrane disappears and the spore is free. During germination the outer membrane of the spore is cast, whilst the inner membrane becomes the new membrane of the new bacterial cell that is formed as a result of the germination.

We may, I think, adduce the fact that a small round object is found in the middle of the spore area, together with the fact that this spore area becomes a body which is endowed with life, in support of the contention that the bacterial cell possesses a nucleus. The various facts that have been brought forward to prove the presence of a nucleus lend mutual support to one another, but it must be confessed that all doubts cannot be regarded as abolished.

A subject worthy of attention is the mode in which large quantities of volutin, oil globules, and glycogen are formed in the cell; and, further, the relatively enormous quantities of sulphur globules that are present in the sulphur bacteria when these organisms are in their prime. So far as the first three reserve materials are concerned, their formation appears to be by direct secretion from the protoplasm. There appear to be no special plastids concerned in the process such as are found in the leaves of plants when starch grains are formed. The presence of special plastids is not necessary even in all cases where starch is formed, so that we need not be surprised that direct secretion from the protoplasm should be the rule in the formation of bacterial reserve matter. In any case, no plastids of any kind have been discovered in bacterial cells. We have seen that the spore is formed by the absorption of matter into a previously prepared area, and it is possible that this method may be the rule in the formation of reserve matter, although it is not probable. The case, however, is different when we come to consider the formation of reserve matters among some of the higher bacteria. The formation of sulphur in the sulphur bacteria follows the absorption of sulphuretted hydrogen, which is oxidized under the influence of living matter into sulphur. This sulphur is stored, but is gradually used up and converted into sulphates. The formation of sulphur is a far less complicated process than that of volutin or glycogen, and its collection appears to be of the nature of secretion into a prepared area, only that the osmotically active substances are not controlled by living matter, as seems probably the case in the formation of the spore. There is invariably found a clear area surrounding each sulphur globule in the cell separating it from the surrounding cytoplasm. When a sulphur globule has been used up by the cell a gap is formed which is larger than the sulphur globule which first occupied it. It is possible, of course, to give another explanation to this fact, such as, for example, that the sulphur globule had contracted after its formation; but it seems to me that the most reasonable explanation is that a space is cleared in the cytoplasm which contains osmotically active substances, and that these have a selective action on the sulphur formed during the first stage in the metabolism of the absorbed sulphur compounds.

In our consideration of the bacillus we must not forget that outside the sharply defined membrane there is a covering, normally very thin, of mucilaginous matter doubtless formed by the degeneration of the outer portion of the membrane. This layer is very difficult to stain, but its existence is made evident when two cells are observed in the toils of division. The two newly separated daughter cells do not appear to be able to effect complete separation for some time after they have come apart and the cause is to be traced to a filament of mucilage which binds them. There seems little reason to doubt that the intervention of unfavourable conditions increases the amount of this substance which is formed and so increases the power of cohesion of the cells. The results which were outlined above as following the frequent subculture of bacterial species were evidently due in the first place to the absence of this mucilaginous substance in any appreciable quantity; and this in its turn caused the absence of cohesion of the cells, which was such a marked feature in the cultures after this treatment. It seems to me that the varying results, which so often accrue when different analysts estimate the disinfecting value of certain commercial fluids, is due to the varying amounts of mucilage outside their membranes which the cells of different cultures of the same species possess.

The intimate structure of the coccus tallies so closely with that of the bacillus that no doubt exists as to the close genetic connexion of the Bacteriaceae and Coccaceae. The differences are only such as are inseparable from the cylindrical form of the one and the round form of the other. The methods of cell division and of spore formation are identical in every respect. The tendency to form groups, either regular or irregular, is not natural to the Coccaceae, and, as I have explained elsewhere, can be eliminated altogether by cultivating under conditions which prevent the formation of this mucilage in any quantity. The demonstration of the vacuolar nature of the cytoplasm has not yet been accomplished. In the formation of the spore the presence of the spore area has been noted, and it has been ascertained that the same stages in the formation of the spore are followed. It is evident that the massing of the cocci into clumps destroys motility, but when this massing together is prevented, as explained, movement follows.

We come next to the third group, of which the genus Spirillum is the most typical member—namely, the Spirillaceae. The intimate structure of the spirillum is on a plan which is totally different from that found in the other two groups. There is an outer limiting membrane, but it is doubtful whether it differs in kind from the inner part of the cell. When cell division takes place the membrane plays no part whatever and the cell behaves as though it were devoid of a membrane altogether. Division takes place by a complete separation of the cell into two completely rounded off portions connected by a thick bridge of mucilaginous matter shaped like a concave lens. The size and form of the cell remain unaltered during the process and the formation of the two daughter cells seems to take place very rapidly. The remainder of the process is merely a gradual separation of the two newly formed cells. These gradually move apart under the influence of the flagella. The lenticular bridge of mucilage under the influence of the tension between the two new cells becomes more and more attenuated until very often the space between the still connected cells is greater than the length of the cells themselves. By staining with fuchsin all stages in this separation can be followed.

Ultimately the connecting cord is broken and the two cells become completely separate. It is evident that the division of the cell is accomplished by simple partition which takes place very rapidly, and there is a complete absence of that elaboration which marks the cell division of the Bacteriaceae and Coccaceae. It has not hitherto been found possible to demonstrate any differentiation in the groundwork of the cell, although it has been stated that a nucleus comparable to the body called by the same name in the bacillus can be distinguished from the reserve products (volutin and oil drops) which are found in the spirillum. The scheme of the structure that prevails in the Spirillaceae is thus markedly different from that which prevails in the other members at present included under Bacteria. The Bacteriaceae and Coccaceae are typical plant groups, but it is not improbable that in the future claims may be advanced by the zoologists for the inclusion of the Spirillaceae in their domain.

Finally, we come to the thread bacteria, which are distinguished from the lower bacteria which we have discussed above, in some forms by the cohesion of the cells to form a community, in other forms by the greater length and size of the cells. The cells of the thread bacteria show no division of labour. The cause of the cohesion of cells in those thread bacteria which consist of cell communities is to be traced to the further development of the mucilaginous covering which we saw was not absent from the lower bacteria. This covering hardens in such organisms, and thus prevents the escape of the dividing cells. We have, therefore, a filament of cells, each cell possessing its own membrane, and the whole row of cells surrounded by an envelope of hardened mucilage. I have established the identity of this substance with the mucilage which surrounds the bacillus by finding its presence between the cells of *Cladothrix dichotoma* as well as on their sides. The individual cells of such organisms as Cladothrix and Crenothrix do not differ in any essential detail from those of the bacillus. So far as the information which we at present possess gives us any clue to affinities it may be said that the iron bacteria as a class are more closely related to the Bacteriaceae, but that the sulphur bacteria adhere more closely to the Spirillaceae, and therefore, to my mind, are as much to be regarded as representatives of the animal as of the plant kingdom.

I have not had any opportunities of examining the internal structure of other genera of the higher bacteria, and unfortunately the amount of knowledge of these forms is very meagre. It is to be hoped that this state of matters will be remedied in the future.

SECTION OF TUBERCULOSIS.

Sir Robert Philip, M.D., LL.D., P.R.C.P.Edin., President.

PRESIDENT'S INTRODUCTORY REMARKS.

This is the first occasion when at a meeting of the British Medical Association a Section has been devoted to tuberculosis. The time at the disposal of the Section is short. I do not propose therefore to occupy more than a very few minutes by way of introduction. The institution of the Section is a fact of much significance and merits emphasis. When the Association met last in Glasgow in 1888 such a Section was undreamt of. Recall how things stood then. The tubercle bacillus had been determined six years before, but little practical heed had been given to the discovery. The first tuberculosis dispensary had just been established and the sanatorium was beginning to be spoken of.

How remarkably the situation has changed since then! The bacillus has been the object of countless researches and forms the centre of a vast literature. The tuberculosis dispensary is an accepted institution throughout the world. It has been reproduced by thousands. Sanatoriums and their congeners, hospitals, tuberculosis schools, and working colonies have sprung up everywhere. In Great Britain the necessity of linking the various institutions has been officially recognized. Tuberculosis schemes, which include the several institutions variously grouped, and a tuberculosis service with duly accredited officers, have been established by statute. Tuberculosis now occupies a place in the group of infectious diseases, and notification of the disease in all its varying forms is compulsory.

During 1921 in England and Wales the number of notifications of tuberculosis amounted to 71,702, and in Scotland 12,524.

On June 1st, 1922, tuberculosis schemes in England and Wales included 381 tuberculosis officers, 441 tuberculosis dispensaries, 84 visiting stations, and 20,395 beds in sanatoriums, hospitals, etc., with 2,664 additional beds in preparation. At the same date in Scotland tuberculosis schemes included 80 tuberculosis officers, 29 tuberculosis dispensaries, and 3,558 beds in sanatoriums, hospitals, etc. In addition to this official provision there are many beds available and much collateral effort on the part of voluntary agencies.

The medical student of to-day receives special training in the subject. At the University of Edinburgh attendance on a course of thirty meetings forms a compulsory part of the medical curriculum. Societies for the study and prevention of tuberculosis have been established in most countries, and an international union embracing representatives of over forty nations is in active being.

A single word as to the main facts concerning mortality from the disease during the period under review. The drop in the death rate in the interval since the Association met last in Glasgow is remarkable and reassuring. During the period from 1881 to 1890 the average number of deaths from all tuberculosis in England was 66,526—that is, 242 per 100,000. In 1919 the number of deaths was 46,310—that is, 126 per 100,000. In Scotland the corresponding figures are: for the period 1881–90, 10,645 deaths—that is, 276 per 100,000; and for 1919, 6,326 deaths—that is, 129 per 100,000. Or stated otherwise, if we take 100 as representing the average death rate from all forms of tuberculosis in each country for the period 1881–90, the mortality for 1919 in the case of England works out at 52 and in the case of Scotland at 47. These striking records are still more remarkable in view of the more accurate and exacting methods of diagnosis which have followed the advance of knowledge regarding the etiology and nature of the disease.

The study of tuberculosis, which in 1888 was considered by many as practically exhausted, flat, dull, and unprofitable, is now alive and progressive. The recognition of the subject at the Annual Meeting of this great Association is therefore abundantly justified.

DISCUSSION ON THE CLINICAL DIFFERENTIATION OF PULMONARY TUBERCULOSIS FROM OTHER RESPIRATORY AFFECTIONS.

OPENING PAPER

BY

E. RIST, M.D.Paris.

Not very many years ago the diagnosis of lung tuberculosis from other respiratory diseases seemed a rather trite and certainly little inspiring subject for discussion. That it should have been chosen as one of the topics of the day at this meeting is in itself sufficient proof that some important changes have taken place in our theoretical and practical knowledge of tuberculosis. Even before the world war premonitory signs of those changes were not lacking, although they were perhaps evident only for the naturally limited circle of medical men who specialized in the scientific study of consumption. But it is certainly due to the experiences of the war that the recognition of an urgent need for remodelling our diagnostic methods has been forced upon the profession as a whole, and, I may add, all over the civilized world.

Not only when the hugest and best-equipped armies which history has ever seen entered the field, but even from the moment they were formed, gathered into camps and barracks, and trained, the problem of tuberculosis had perforce to be considered in a new light by the medical officers responsible for the selection of men, for the physical efficiency of recruits, and, last but not least, for the future liabilities of the respective Governments under the Acts providing for pension and insurance in favour of soldiers disabled by disease. Consumption is a chronic malady. Unfortunately it cannot be cured by any of those radical, immediate, clear-cut methods which make it an urgent duty to diagnose early and exactly and to treat deliberately and promptly some of the diseases which modern medicine can to a certain extent boast her ability to conquer: diphtheria, malaria, syphilis, appendicitis, for example. Although we were theoretically prompted by our teachers to look for early signs and not to delay treatment, in everyday

practice the "wait and see" policy was almost universally applied to lung tuberculosis. Individual cases were for protracted periods regarded as merely suspicious. Treatment, which so often entails a total change in the material and moral life of the patient, with considerable sacrifices and expenditure, was advised late—in many instances too late. We all know that, by another school of physicians, it was—and still is—often imposed uselessly on people who do not need it, on the ground that it is better to prevent a possible outbreak of tuberculosis than to try to arrest it when fully developed.

The necessities of war have put before us the duty of rapid, well-founded, uncompromising decision in matters of tuberculosis diagnosis. No belligerent State could afford to keep thousands of men under expectant observation in order to decide whether, after several weeks or months, they would be rejected from or admitted into the ranks. No belligerent State could afford to admit into the army men who not only would be unfit to serve but would spread contagion among their comrades. Nor could they afford to deprive themselves of the services of any man who was really fit to be a soldier, and to reject him on the ground of some spurious suspicions which events would prove unfounded. For obvious reasons considerations of the same kind had to be borne in mind in the case of men who fell ill when serving. If really tuberculous, it was in the interest of the army as well as of the State that a correct diagnosis should be made as soon as possible, in order that they should be discharged and treated without delay. If not tuberculous, the same joint interests imposed upon the medical officers the duty of making an accurate and early diagnosis, in order that mild, transient, and curable non-tuberculous respiratory disease should not have as its consequence discharge and pension, and that more serious non-tuberculous disease should be, whenever amenable to cure, treated judiciously.

I am sure nobody will feel shocked or surprised if I say that, as a rule, medical officers were not well prepared, in the early days of the war, to meet these new duties. It soon became evident that mistaken diagnoses in the matter of long tuberculosis were all too numerous. The mistakes were not always in the same direction. Of course—and this had been generally foreseen—many cases of consumption were overlooked. But the fact that a considerable number of cases labelled at first "tuberculosis" and disposed of as such were ultimately proved not to be tuberculous at all was certainly quite unexpected, even to the majority of tuberculosis experts.

That the proportion of errors in the routine diagnosis of lung tuberculosis is apt to be very large, some figures, which I intend to give later, will illustrate. At the present time my purpose is only to remind you how the experiences of the war convinced the majority of us that there was something wrong in our everyday methods of discriminating tuberculosis from other diseases and more especially from other respiratory diseases.

NEW POINTS OF VIEW CONCERNING PULMONARY TUBERCULOSIS.

As a matter of fact, new and important knowledge had been slowly gained in various fields of pathology and clinical investigation, but we had failed in adjusting our diagnostic procedures to this knowledge. What I mean can be conveniently summarized under four heads.

1. *The Skin Reaction.*

The skin reaction to tuberculin has proved that tuberculous infection, at least among civilized communities, is almost universal. We are now aware that the first infection is generally accomplished during the first fifteen or eighteen years of life, but that it is followed by tuberculous disease in only a small number of exposed individuals. That a mild first infection confers an immunity which protects the majority of adults against further infection and disease is a well-ascertained and generally admitted fact. It seems most probable that the mildness of a primary infection is not due to a hypothetical attenuated virulence but to the small number of germs, heavy infections determining severe, generalized, and, as a rule, lethal forms of tuberculosis in children as well as in adults hitherto non-exposed—as, for instance, natives of non-civilized countries. A positive skin reaction, therefore, cannot be considered as a diagnostic sign of tuberculous disease, but only as an index of tuberculous infection. In other words, we must discriminate between infection, which is almost universal in the civilized adult, and disease. The earliest beginnings of disease are not, as was supposed, coincident with those of infection. The once prevalent idea that early diagnosis—and, as its consequence, early treatment—should endeavour to discover and to ascertain the initial steps of infection has proved a fallacy. We are now able, thanks to the skin reaction, to recognize tuberculous infection almost at its beginning. But, contrary to the expectation of fifteen years ago, we are led to admit that the infected individuals which the von Pirquet test discovers are more protected against subsequent tuberculous disease than the non-infected. The whole problem of early detection of lung tuberculosis must consequently be considered from an entirely different point of view.

2. *The X Rays as an Aid to Diagnosis.*

X-ray examination of the chest has—slowly, indeed, but persistently and successfully—gained its legitimate place among our methods of physical diagnosis. The instruments and the technique have been improved to a considerable degree. An increasing number of physicians have taken up the study of fluoroscopy and radiography. Of course some of us are still reluctant, and ask for *x*-ray information only as a last resource in exceptional cases. A century ago auscultation was also regarded as a last resource by many who, apparently, were not mindful that

> "There are more things in heaven and earth, Horatio,
> Than are dreamt of in your philosophy."

To appreciate the real value of a new method of physical diagnosis, there is only one way, and that is to use it regularly and persistently together with the classical methods, in ordinary and simple not less than in rare and complicated cases. Not only the so-called *x*-ray expert, but the clinician as well should master the technique of fluoroscopy and train himself in the reading of plates, constantly controlling the information so obtained by the ordinary physical examination and by *post-mortem* findings. Fortunately this is being done more and more; there are already physicians who think that a chest has not been completely examined if it has not been *x*-rayed, and who act accordingly. I may be perhaps allowed to confess that, for my part, I have adhered to that principle these last fifteen years, and that all the patients whom I have examined, either in hospital or dispensary work or in private practice, have been *x*-rayed by myself.

No wonder if the prolonged use of the fluoroscope and the plate has gradually modified our views concerning the practical value of other methods of physical examination. One has to admit that percussion and auscultation have their limitations and pitfalls; one cannot deny that the *x*-rays are able to inform us rightly in many cases when the older methods leave us doubtful or mislead us. Even when the rays only confirm what auscultation and percussion reveal, they help us by adding weight to our argument. But when the results of *x*-ray information and of classical physical examination are contradictory, which is often the case, we ought not simply to disregard the first, but to reconsider the second, in the light of that contradiction. This practice has led to a great deal of new knowledge. I may mention the discovery by the fluoroscope of deep-seated lesions which the ear is unable to detect, the now well-established fact that cavitation is generally much earlier and much more frequent than was supposed, or that consolidation in tuberculosis is, as a rule, more extended than one would think, if one were guided only by the stethoscope. On the other hand, there are cases when râles and other adventitious sounds suggesting an extended parenchymatous lesion do not coincide with any abnormal *x*-ray finding, and where consequently we have to ask ourselves whether the acoustic phenomena are not merely symptomatic of bronchial catarrh, without any alteration of the lung tissue. To interpret rightly the sequence of events in the evolution of lung tuberculosis we must therefore take into account the treasure of valuable information with which the *x*-ray exploration of sound and diseased chests has enriched our knowledge. At the same time we must take advantage of the powerful aid which the rays have brought us in the diagnosis of non-tuberculous chest disease.

3. *Examination of Sputum.*

There has been lately a general tendency to attach much more importance than one did before to the demonstration of tubercle bacilli in the sputum of consumptives. Our methods of detection have been improved. Homogenization of the sputum, inoculation of the guinea-pig, are now widely used. Laboratories are at the disposal of the greater part of the profession. A complete examination of a supposed tuberculous patient implies repeated and conscientious sputum examination. The result of this close association between the bacteriological laboratory and the bedside or dispensary practice has been to show us that bacilli appear in the sputum much earlier than was admitted before, and consequently to throw some doubt on a diagnosis of tuberculosis which has not been corroborated by the demonstration of the causal micro-organism in the morbid secretion. Not many years ago, when physical examination elicited the signs of a lung lesion, and when some of the symptoms of consumption, such as fever, haemorrhage, coughing, and wasting were present, one thought oneself perfectly justified in considering the diagnosis of tuberculosis certain, even if no bacilli could be demonstrated; as a matter of fact, one did not always take the trouble of looking for them. We are less inclined to do so now. An open lung lesion where no bacilli can be found we regard as a problem which cannot always be easily solved. It obliges us to discuss the possibility of a disease other than tuberculosis. I shall even go farther: it makes that possibility very plausible.

4. *Infections of the Upper Respiratory Passages.*

The nose and throat specialist has become an appreciated guest at the medical symposium. He has improved his instruments, his technique, and his knowledge. The diseases of the upper respiratory passages have asserted their frequency and their importance. The nasal cavities belong to the breathing apparatus as well as the lungs. Their infections, catarrhs, and mechanical obstructions influence the bronchial tree, and even the alveolar tissue, quite as naturally as the maladies of the urethra and prostate influence the bladder and kidneys. There is no real frontier between the province of the nose and throat specialist and the empire of the lung expert. The more one studies bronchial and lung pathology the more one gets convinced of the interdependence of all the parts of the respiratory tract. The realization of this apparently so self-evident but unfortunately so long overlooked truth has contributed to no small extent to changing our points of view regarding the diagnosis of lung tuberculosis.

The Modern Conception of Tuberculosis Diagnosis.

I think the views which have just been summarized are more or less shared by an increasing number of those physicians who have specialized in the study and diagnosis of lung tuberculosis. I say more or less, because, according to their personal tendencies, they would naturally lay more stress on one or two of my four points rather than on all of them. In order to ascertain to what extent the conception—which I would venture to call the modern conception of tuberculosis diagnosis—differs from the traditional, one has only to study the statistical figures which have been published recently by those well-equipped clearing stations or hospitals where, in many countries, service or ex-service men are now submitted to observation before discharge, admission on the pension list, or treatment. I shall quote a few instances.

In 1916 I was conducting, in the capacity of an army consultant, a tuberculosis clearing station at Compiègne. Out of 192 men sent in with a diagnosis of lung tuberculosis from various army units, ambulances, or hospitals, and kept under observation, this diagnosis was found to be justified in 53 cases only (or 27·5 per cent.). I may mention that this figure includes arrested and healed as well as active tuberculosis; 139 cases (or 72·5 per cent.) were undoubtedly non-tuberculous.

During the summer and autumn of 1918 I acted in the same capacity at the Laënnec Hospital, for the troops stationed in Paris. Of 382 men examined, 37 only (10·8 per cent.) had lung tuberculosis; 22 (6·4 per cent.) were not definitely diagnosed because the time during which they were kept under observation was too short; 283 (82·7 per cent.) were certainly not tuberculous. During the winter 1919-20, 119 men who had been proposed by medical officers for pension on the ground of lung tuberculosis were referred to me and taken into my hospital wards: 47 per cent. were tuberculous, 53 per cent. were not.

My friend Professor Sergent acted, during the greater part of the war, as the chief physician of a tuberculosis clearing hospital for French soldiers. Out of 1,719 men, he found 542 (31 per cent.) who were not tuberculous. Among the 69 per cent. tuberculous men, more than one-half had healed lesions which did not justify a discharge from service.

Dr. Dickinson, tuberculosis officer of Newcastle-upon-Tyne, very kindly supplied me with the following information concerning the ex-service men who applied in Newcastle for sanatorium benefit between the years 1913 and 1919. Out of 287, 167 had bacilli in their sputum, and 120 had none. Of those 120 men, 91 (or 75·8 per cent.) were ultimately proved to be non-tuberculous. Among the 167 who had bacilli 53 died; among the 120 whose sputum was free from bacilli 4 only died; the causes of death in these 4 cases were respectively bronchiectasis, influenza, sarcoma of the lung, disseminated tuberculosis.

In the report of the Public Health Committee of the London County Council for 1921 we read the result of observation of 158 ex-service and insured London cases sent, for diagnostic purposes, to the Brompton and City of London Chest Hospitals during the year 1920. Of 158 cases, 124 (or 78·4 per cent.) were reported as not suffering from tuberculosis. Of the 34 cases diagnosed as tuberculous 10 were considered to be "arrested" cases not requiring treatment; only 24 (or 15 per cent.) of the 158 cases being recommended for treatment.

I have limited my quotations to Great Britain and France. But I could have given you figures quite similar from Swiss, Italian, or American sources. The discrepancies which they illustrate are universal. Of course, it would be absurd to imagine that they are peculiar to the limited group of service and ex-service men. The diagnostic criteria which are commonly applied to those men before they are referred to special diagnostic centres are quite as widely used in ordinary civilian practice, and they lead to a similar proportion of mistakes. At the out-patient consultation for lung disease which I conduct at the Laënnec Hospital alongside with my in-patient department, I find that 62 per cent. of the cases referred to me by tuberculosis dispensaries or private practitioners as consumptive suffer from diseases which have nothing to do with tuberculosis. A large number of them have nevertheless been treated, sometimes for many years, as if they were tuberculous; they have been the occasion of considerable—and perfectly useless—expenditure of money at their own or at public expense; they have been submitted to all the hardships, restrictions, moral and material sacrifices which attend the life of a tuberculous patient; and finally they have not even received the proper treatment which the real disease they suffered from required.

Diseases Mistaken for Pulmonary Tuberculosis.

The question we have now to ask ourselves is: Which are the commonest diseases which are so often mistaken for lung tuberculosis? They fall under three heads.

In the first place, we find ailments which are not caused by morbid conditions of the respiratory system, but have some symptoms in common with those of such conditions, or determine secondary lesions of the lung or pleura. I may briefly mention the diseases of the cardio-vascular apparatus: dyspnoea, coughing, thoracic pain, haemoptysis belong to their symptomatology. They are not infrequently complicated with bronchitis, pleural effusions, passive congestion, oedema of the lungs, or lung embolism; these, of course, are manifested by physical signs which are apt to mislead one when the symptomatology of the causal disease remains obscure as is sometimes the case. Let me remind you, for instance, of the blood-spitting which so often occurs in mitral stenosis, or of the lung embolism concurrent with prolonged fever which is one of the features of malignant endocarditis. Diseases which are associated with wasting, loss of appetite, and progressive weakness, such as cancer of the digestive apparatus, conditions which determine loss of weight, unstability of the body temperature, tachycardia, like Graves's disease, should also be taken into account. But, vast and interesting though this category undoubtedly is, it does not fall within the scope of the present discussion.

We are properly concerned, on the contrary, with the second group—namely, non-tuberculous diseases of the intrathoracic respiratory apparatus, whose signs and symptoms are for the greater part identical with those of the manifold forms of lung tuberculosis. Some are exceptionally met with:

Syphilis of the lung, for instance, has absolutely no signs of its own, and therefore it is almost regularly regarded as tuberculosis, until the repeatedly negative examination of the sputum for bacilli opens the door for suspicion into the mind of the physician. In many published cases the surprisingly rapid cure of the lung ailment under the influence of specific treatment given for some cutaneous tertiary lesions was the key to the recognition of its real nature.

Echinococcus of the lung also belongs to the rarer diseases, at least in this country. It is much less frequent in France, and in the Argentine Republic or in Australia it is one of the commonest chronic maladies of the chest. Haemoptysis may be the only symptom during the initial period. If the cyst is located in the vicinity of the pleura, it causes a pleural effusion which simulates tuberculous pleurisy. If it has emptied into a bronchus, the signs are those of a lung cavity, and attributed for this reason to tuberculosis, unless one has been fortunate enough to observe the pathognomonic membranes when they were expectorated. The absence of tubercle bacilli in the sputum together with the characteristic ball-like appearance of the cyst on the fluoroscope or the plate, and the specific complement fixation are the only means to come to a correct diagnosis.

Cancer of the lung or bronchi is not so exceptional. The diagnosis is always a difficult one. In a recent paper dealing with the 29 cases of bronchial or pulmonary malignant growth which came during the last twenty years to autopsy at the University Hospital of Geneva, Cramer, Saloz, and Mlle Cottin showed that the clinical diagnosis had been the right one in only 20 per cent. In five instances the mistake was due to an unjustified admission of tuberculosis, based on physical signs of consolidation or cavity, dyspnoea, coughing, wasting and haemoptysis. Almost all the cases of lung cancer which I have observed had been previously regarded as cases of consumption. One must not rely upon the finding of neoplastic cells in the sputum, nor upon their demonstration in the pleural effusions which frequently complicate the new growth. They are seldom to be found under the microscope, and sometimes quite impossible to differentiate from modified endothelial cells. Only careful *x*-ray examination and the absence of bacilli in the sputum can lead to an accurate diagnosis, if there are no definite arguments such as those which are sometimes to be found in the microscopical examination of a subcutaneous metastatic nodule. Even the demonstration of tubercle bacilli in the sputum is not always to be interpreted against cancer, as one knows cases where a malignant growth has originated on the walls of a tuberculous lung cavity.

Bronchiectasis is a morbid condition more commonly observed than either syphilis, echinococcus, or cancer of the lung. If it were always manifested by the symptoms described in our textbooks, especially by the characteristic and abundant morning discharge of offensive purulent secretion, it would undoubtedly be diagnosed correctly in the majority of cases. But one must remember that, for many years, a bronchiectasis may determine a secretion which is not only non-offensive, but intermittent, periods of apparent inactivity alternating with periods of abundant secretion coinciding with irregular fever. Sudden and profuse haemorrhage as well as light and prolonged expectoration of blood are also a by no means unusual feature of the disease. The physical signs are those of focal consolidation with a central cavitation. The classical axiom that apical cavities are tuberculous, whereas cavities located at the base speak in favour of bronchiectasis, is one of those traditional golden rules which are just approximate enough to be dangerously misleading. As a matter of fact, apical bronchiectases are not more exceptional than basal tuberculous cavities, as pathologists well know. I have recently compiled all the cases of bronchiectasis which came under my observation at the Laënnec Hospital since June, 1919. Out of a total of 22, 14 had been mistaken for consumption and treated as such. Some of them had spent years under sanatorium cure. If more stress had been laid on the permanent absence of tubercle bacilli in the sputum, and if the past history of the patients had been more carefully investigated, I think this proportion of errors could have been very materially reduced. To ascertain, in the anamnesis of a patient who expectorates non-bacillary purulent sputum, that his trouble has begun after he has had a typical pneumonia, or, in childhood, a bronchopneumonia complicating diphtheria, measles, or whooping-cough, is, in my opinion, more important than to verify if his expectoration has the macroscopic aspect so complacently described by our classics. A history of foreign body aspirated into a bronchus should also be searched for.

Purulent non-tuberculous pleurisy, whether located in an interlobar fissure or encysted in some other part of the pleural cavity, is also one of the diseases which may for a long time escape recognition because they are mistaken for tuberculosis. An empyema gives focal signs and general symptoms such as fever and wasting; when ruptured into a bronchus, it determines purulent expectoration. I am sorry to say that these manifestations are deemed quite satisfactory arguments by many a medical man, to label the case "consumption" without any further investigation.

One could easily lengthen the list of pulmonary and pleural conditions which in daily practice one has the opportunity to discriminate from pulmonary tuberculosis. But, at the risk of being blamed for some omission, I hasten to draw your attention to the third class of ailments which are apt to bias our diagnosis—namely, the *morbid conditions of the upper respiratory passages*. Experience has shown that they are to be considered as a very frequent, if not as the most frequent occasion of error. The common cold—or, to give it its pompous scientific names, acute coryza or acute rhinitis—is a mild infection of the nasal cavities which, as everybody knows, easily spreads to the pharynx, the larynx, the trachea, and the larger bronchi, determining coughing and expectoration. It is a strange fact that one generally overlooks the very plausible probability that chronic infectious conditions of the upper air passages should do the same. Now the labours of our friend, the nose and throat specialist, have taught us that there is quite a variety of those chronic conditions, and that they are exceedingly common: antrum suppurations, for instance, or purulent discharge from the ethmoidal, frontal, or sphenoidal cavities. Adenoids are special to childhood, but they often persist in the adult, as well as hypertrophy of the pharyngeal, lingual, or palatine tonsil. Atrophy of the turbinates is not always associated with its signal symptom ozaena, but it is always determined by a chronic infection whatever the causal micro-organism may be. More frequent still are those minor abnormalities which are not infectious by nature but which give occasion to benign infectious catarrhs, such as hypertrophied turbinates or deflected septa.

Curiously enough, up to recent times, whenever the possibility of an influence exerted by nasal abnormalities on the deeper respiratory organs was thought of, one always considered this influence as a merely mechanical one. It was generally admitted that nasal obstruction is able to disturb the functional activity of the lung, and, by so doing, to create a predisposition for tuberculosis, or to produce, as in Kroenig's theory, an apical collapse simulating early tuberculosis. Outside of nasal obstruction one could imagine no such influence. The infectious element was entirely ignored. To explain this indifferent attitude of the profession, one must perhaps take into account the indifference with which the patients themselves regard the conditions I have just alluded to, even when they have been subject for years to chronic catarrhal or purulent rhino-pharyngeal discharge. They very often get so accustomed to this ailment that they do not even mention it to the physician whom they consult, complaining only of coughing. And yet their habitual coughing may be nothing else but one of the symptoms of a simple chronic bronchitis due to their chronic nasal catarrh, exactly in the same way as acute simple bronchitis originates from an acute common cold.

Even the nose and throat specialists are often blind to the fact.

"O fortunatos nimium, sua si bona norint!"

as Virgil said of the labourers in the fields. When one has had one's attention drawn to this connection between infections of the upper respiratory organs and chronic coughs with

occasional signs of disseminated or localized bronchitis, it is quite surprising how often the curetting of an antrum, the withdrawal of an hypertrophied turbinate bone, the removal of adenoids or of hypertrophied tonsils puts an end to a long story of apical catarrh or of suspicious bronchitis or of incipient tuberculosis. Over 50 per cent. of the cases which I reported as non-tuberculous after observation in the diagnostic clearing stations I conducted during and after the war belonged to that class of patients. Since I first published my views on this subject in 1916 they have been confirmed by many physicians, among whom I would like to mention especially Professor Sergent in France and Dr. Gerald Webb in the United States.

REASONS FOR MISTAKES IN DIAGNOSIS.

There are many reasons to account for the large proportion of mistakes which are committed in the discrimination of lung tuberculosis from other respiratory disease. It is not my intention to mention them all. But I may be allowed to deal briefly with one or two. In the first place, the importance of an early diagnosis of tuberculosis has been urged upon us by our teachers, by public health authorities, and even by public opinion, with such force and persistency, that however beneficial and well meant this urging has been and still is, it has to some extent biased our clinical judgement concerning individual cases. Even that admirable institution, the dispensary, is in many places organized as if the patient who enters it for the first time could not be suffering from any other disease than tuberculosis. The printed forms where the histories, the symptoms, and the results of physical examination are to be consigned, direct unconsciously our thoughts towards one hypothesis only—namely, tuberculosis. And it seems as if the medical officer had only to decide whether the case is one of simple exposure, or of incipient, confirmed, moderately active, progressive, arrested, or healed tuberculosis. If the physician is not a very experienced clinician, using all the diagnostic means at his disposal in a well-equipped institution, if he has not a strong character and an active, investigating mind, he will naturally be inclined to follow the line of least resistance which is so carefully and methodically drawn before him.

In the second place, I should like to say that, in my opinion, too great importance is generally given to symptoms *versus* signs as a positive basis for a diagnosis of tuberculosis. Symptoms have their due place in the mental process which leads to a diagnosis. I am in no way inclined to minimize their value, which is great. They are the indispensable starting point of our journey. But one is too apt to forget that the journey is quite as indispensable. As a matter of fact there is not a single symptom, not even a single association of symptoms, which tuberculosis has not in common with other diseases. Loss of appetite, weight, or strength, shortness of breath, coughing, expectoration, fever—all belong to an extraordinarily large group of morbid conditions. Even the so-called pathognomonic subfebrile temperatures after exertion are commonly observed in all sorts of chronic or subacute, more or less latent infection (chronic appendicitis, for instance, or an inflamed Fallopian tube, or a suppurating antrum). Nor is it always absent in conditions which are not infectious at all, as in Graves's disease. Haemoptysis is very generally considered as an almost infallible positive sign of tuberculosis, and yet we know that it occurs in every focal non-tuberculous disease of the lung: syphilis, cancer, bronchiectasis, gangrene and abscess of the lung, and echinococcus have this characteristic sign in common with tuberculosis. We also know its diagnostic value in mitral stenosis and in malignant endocarditis, not to speak of the sometimes very profuse haemorrhages which signalize papillomatous or epitheliomatous new growths of the larynx or varicose veins in the trachea or the pharynx.

The first step after analysing the symptoms is to look for positive signs of a localized lung lesion. And here begins properly the task of the physician. Percussion, auscultation, and the other methods of physical examination must be applied without any bias. I mean that the physician should not, at this step, ask himself the question: Has this patient tuberculosis? But simply: Is there, either in the lung or elsewhere, a localized lesion which may account for the symptoms observed? I need not emphasize again the value of an *x*-ray examination of the chest, to show whether such a lesion can be demonstrated or not. If it cannot be demonstrated in the lung, then let us look for it in some other part of the body. And here, after due consideration of the cardio-vascular and other systems, comes in the careful examination of the upper air passages by an expert. It will often, I dare say, prove fruitful of a correct diagnosis.

On the contrary, if the signs of a localized lesion—or of several localized lesions—are found in the lung, if the gross anatomic characteristics of this lesion have been ascertained by an *x*-ray investigation, we may proceed to diagnose the specific nature of the lesion, by using the tests which are at our disposal for that purpose. As regards tuberculosis, there is only one positive test—namely, the finding of tubercle bacilli in the sputum. It must be used with a persevering tenacity. One negative result means nothing. I would not like to give a limit to the number of examinations which are necessary. But I think ten negative results mean a lot, if the search for bacilli has been prolonged, if the sputum has been homogenized, and if animal inoculation has not been neglected. May I add that *one positive result* may mean very little? There are stupid, but not always quite unavoidable, causes of error, which every trained laboratory worker knows, and there may be also intentional deceit from the part of the patient, if he is eager to get an undeserved pension or compensation.

It would be absurd to contend that the diagnosis of tuberculosis is justified only when bacilli have been demonstrated in the sputum. But the more one studies this question of the discrimination of tuberculosis from other disease, and the more one has opportunities to follow up patients for long periods of time, the more one sees how very frequent indeed are the cases, even the quite early cases, of real tuberculosis where bacilli are found in the sputum, when they are carefully looked for. I do not mean to say that a patient with positive signs of a localized lung lesion and no demonstrable bacilli is certainly not tuberculous. I say that our duty in the absence of bacilli, is to endeavour earnestly to ascertain whether some disease other than tuberculosis may not account for the symptoms and signs observed. Only if we do not succeed in our investigation are we entitled to admit the diagnosis of tuberculosis, so to say, by exclusion and as the most practical working hypothesis.

DISCUSSION.

Dr. MARCUS PATERSON (Colindale Hospital, Hendon) said: I have two criticisms to make on Professor Rist's paper, otherwise I am in agreement with his remarks. In the first place, he told us how to establish the existence of tuberculosis but did not suggest any method of determining whether the disease in a non-febrile patient is active or not. He states that "new and important knowledge has slowly been gained on this point, but we have failed in adapting our diagnostic procedure to this knowledge." He does not appear to realize that we have now the auto-inoculation test, which, especially if pressed to the point of reaction in a case with positive sputum, is of great value. Certain symptoms which give good warning are shown by a patient when the reaction is approaching—namely, in the following order: insomnia, anorexia, slight malaise, and marked deviations in temperature. The more certain one is that the case is not tuberculous, the more the test can be pressed. Then, again, there is the complement fixation test to prove infection. To some extent this test also indicates activity.

I agree with him, from my experience at Colindale, that a mild first infection confers immunity. I do not know if any facts bearing on this have yet been published, but I have, at the request of Sir James Kingston Fowler, been keeping records of signs of previous tuberculous disease—for example, scars of operation on glands, etc.—in patients suffering from advanced tuberculosis admitted to Colindale Hospital, and under 5 per cent. bear such evidence.

The generally accepted statement that persons dying of tuberculosis have evidence of old lesions in the abdominal glands has been tested in some two hundred necropsies, and is found to be quite untrue. Tuberculous glands are present in only a very few cases. I do not deny that they may be present in patients dying of other affections, but they are very uncommon in persons who have died of pulmonary tuberculosis.

I should like to quote some figures to indicate the necessity of repeated examination of sputum when the first examination is negative. These results refer to sputum passed by a medical officer as such, and not simply saliva sent by the patient. Excluding cases which had abundant bacilli on

the first two examinations, and also cases which are negative up to ten examinations or more, and taking only those sputa from patients with very extensive signs we find, after careful search, that two out of three examinations are negative. In this advanced type of case it was also found that specimens which had not been passed in the first place by the medical officer as actually sputum gave a very large percentage of negative results. Professor Rist says that one positive sputum means very little—I would say it means nothing. To illustrate my meaning I might, if time permitted, quote in full six cases who died and in whom there was no macroscopic evidence *post mortem* of any pulmonary tuberculosis whatever. In all of these cases a positive sputum had been returned. I would suggest, therefore, that there must be many similar mistakes, as it must be a rare thing for a man not suffering from pulmonary tuberculosis to die in a tuberculosis institution. For the six cases mentioned there are, therefore, probably many hundreds who have thus been labelled tuberculosis and who do not die of it at all. The greatest care should be taken in sending specimens to a laboratory not to mix names. It is obvious, therefore, that a positive sputum should always be confirmed before a man is labelled tuberculosis. All the above six cases had been through several institutions and treated at great cost. It often happens that a man once labelled tuberculosis is never allowed to have any other illness. In one of these cases the radiologist reported that there was definite and active tuberculosis in the lung.

Professor S. LYLE CUMMINS (Cardiff) said: It is a great pleasure to hear from the lips of such an authority as Dr. Rist this frank admission of the difficulties that exist in what is often thought a relatively simple diagnostic problem. To me it is a source of particular interest and satisfaction because it has been urged by several critics of the new Diploma of Tuberculosis which we have started in Wales that it is quite unnecessary to attempt to produce specialists in connexion with a disease which is so well comprehended by the average doctor. Those who agree with Dr. Rist will readily admit that without very special training and experience, and without access to the aids to diagnosis afforded by good radiology and good laboratory facilities, even the very capable and careful medical man may easily become involved in serious error. The remedy lies in closer study of modern methods and in the taking of special courses of instruction in tuberculosis; and it was with the idea of encouraging those likely to have to deal with tuberculosis to take such courses that the tuberculous diseases diploma was initiated. There is one readily accessible clinical aid to diagnosis of which Dr. Rist has made no mention, but I think it of sufficient importance to deserve special consideration; I refer to the application of Dreyer's physical standards to routine examination of cases suspected of pulmonary tuberculosis. The use of the spirometer is not as general as it should be, and its use, combined with the recording of trunk length; chest measurement, and weight, with the aid of Dreyer's tables, need only be tried to be appreciated. As an index of the progress of cases it is, too, of great value. Evidence for the applicability of these standards to tuberculosis of the lungs will be found in the paper by Dreyer and Burrell, and a more recent communication by Cameron. My own experience with these standards, though the cases do not yet amount to a sufficient number to justify publication, is most favourable.

Perhaps because of the limitations of his title to the clinical differentiation of the disease, Dr. Rist has omitted all mention of serological reactions as aids to diagnosis. The great difficulty about these tests lies in the fact, to which Dr. Rist has so effectively called attention, that we have to bear in mind the practical difference between infection and disease. The aim of the physician is to diagnose disease, not merely to detect infection; and serological tests share with the tuberculin tests the disadvantage that they are liable to be too delicate for this purpose, giving a positive response in a good many cases of infection that may never reach the stage of actual disease. Still recent reports on the complement deviation test give reason to hope for great help in the diagnosis of active tuberculosis by its means. It is the fashion of the moment to regard the opsonic index of Wright as of no account, chiefly, I think, because the test is too difficult for the busy practitioner and too uncertain in its results to appeal to the laboratory worker. Its uncertainty depends upon the numerous factors that underlie it. These factors are very little understood, but they are worth studying. For a disease in which the amount of infection is of such paramount importance the fact that the opsonic index gives a quantitative expression of one of the responses to infection gives it a claim to consideration. Where the test has been carefully applied it has given interesting results, notably in the hands of Dr. Marcus Paterson. I have been engaged for the past two years in re-examining the factors underlying this test, and I have to admit that they are very obscure. I am convinced, however, that the test, in the form originally devised by Wright, is of very definite value, and I would urge those workers in well-equipped sanatoriums with clinical laboratories at their disposal to reconsider it if only to the extent of accumulating records by which its value in differential diagnosis may be finally judged.

Dr. IAN STRUTHERS STEWART (Nordrach-on-Dee, Banchory) said: I have listened with much interest and appreciation to Dr. Rist's paper and am in entire agreement with him that it is essential for the physician dealing with the treatment of tuberculous patients to have a thorough knowledge of the use of *x* rays for the purposes of diagnosis. It would appear to me that at present there is a tendency to place too much reliance on the negative or print and to neglect the radioscopic examination. It may also be that we are apt to read too much into the lights and shadows of the radiogram, as after all we are only dealing with varying intensities of light, and many things cause shadows in the lung tissue other than tuberculosis. Then, again, I do not consider it possible in the majority of cases to differentiate by *x* rays alone between active and quiescent tuberculous disease. That disease of the upper air passages has a profound effect on disease of the lungs I consider to be amply proved. This is frequently to be observed in cases of pulmonary tubercle who develop a simple nasal catarrh, accompanied by an increased intensity of the physical signs in the lungs. That haemoptysis is not always symptomatic of tuberculosis of the lungs is shown by the frequency with which this symptom is met following trauma of the chest wall. In many such cases no symptoms or signs of tuberculosis could be found, and the subsequent history has proved the accuracy of the diagnosis. These cases are of special interest to the pensions medical officers. The question of diagnosis between active and quiescent disease often necessitates the continued observation of the patient for a week or more, and resolves itself largely into the question of whether toxaemia is present or not, irrespective of the intensity or extent of the physical signs. Sarcoma or endothelioma of the lung may simulate pulmonary tuberculosis both as regards the character and the distribution of the physical signs. The presence of a bilateral pleural effusion should raise the suspicion of malignant disease in the thorax.

Dr. JAMES CROCKET (Lecturer on Tuberculosis, University of Glasgow) said: I appreciate Dr. Rist's paper, but feel that there is another side to the picture. Undoubtedly many cases are diagnosed as tuberculous that do not suffer from that disease. Many more are diagnosed and treated for other conditions who actually suffer from pulmonary tuberculosis. Out of 1,100 cases that had been under treatment at Bridge of Weir Sanatorium during the past three or four years 168 had been discharged as "not improved." The majority of these cases are by this time dead. Investigating the history of those patients I find that only 31 had been known to be suffering from pulmonary tuberculosis previous to admission. The remaining 137 cases had been under treatment for from one to fifteen years for other conditions, particularly bronchitis, influenza, anaemia, laryngitis, rheumatism, and neurasthenia. That they had suffered from phthisis during this period there is little doubt. To miss tuberculosis when it is present is, in my opinion, a much more serious thing for the patient than to diagnose tuberculosis and treat the patient for that when it is not there. Sputum examination is not to be depended upon in diagnosis. Out of 2,204 cases tubercle bacilli were only found on the first examination in 60.9 per cent. Patients at present in residence whose sputum has been examined give interesting figures. One examined fifty-seven times, only showed tubercle bacilli on five occasions; another, forty-three times, on four occasions; another, twenty-seven times, on two occasions. One patient previously in the sanatorium, with numerous tubercle bacilli in the sputum, was readmitted recently on account of an exacerbation of the disease. Her sputum has been examined since readmission fifteen times with a negative result. The von Pirquet test has been found far from satisfactory, and in

my experience is not a very sensitive test. Out of 700 undoubtedly tuberculous cases positive results were only got in 80 per cent. of the cases. The complement fixation test has been used, but with rather disappointing results. Dealing with several hundred cases I got 90.5 per cent. positive; 9.5 per cent. undoubtedly tuberculous cases gave negative results. As controls the bloods of 67 epileptic patients were examined, and of that number 12 per cent. were positive. I do not object to 9 per cent. of the tuberculous cases giving a negative reaction, but do not like 12 per cent. of the control cases, undoubtedly not having clinical or obvious tuberculosis, giving a positive reaction.

Dr. A. FERGUS HEWAT (Edinburgh) said: Personally I have found Professor Rist's paper most useful in giving clear lines to this vital question of differential diagnosis. My interest in this question of differential diagnosis as it affects soldiers and pensioners began while in charge of fifty beds for tuberculous cases in the army, and has been continued among pensioners sent to the special hospital arranged by the Ministry of Pensions, on the advice of Sir Robert Philip, for such cases. The problem we have to face amongst such patients is not the old well-discussed question of early diagnosis, but rather the correct valuation of symptoms, signs, and other phenomena which have gone on for months—in many cases years. As one writer puts it: "It is the grouping of the elements rather than the mere presence or absence of elements."

It may be of some interest to members of this Section if I give a few figures from Kingscroft Ministry of Pensions Hospital for 1921. In that year 144 cases were admitted. Practically all of them suffered from pulmonary symptoms which caused them to be referred to this hospital. In 66 of these cases a definite statement was made on their admission form that pulmonary tuberculosis was suspected. This tentative diagnosis had been formed after repeated medical examinations. In 42 of these cases I felt justified in confirming the tentative finding; in 10 of these confirmation was made certain by the presence of tubercle bacilli in the sputum. The remaining 32 included cases classified as chronic pulmonary tuberculosis with bronchitis and emphysema, a case of glands in the neck, and a tuberculous hip. Most of the cases were very chronic in type, and the associated chronic bronchitis and emphysema was the obvious crippling disability and the label under which the patient was in receipt of pension. Fifteen other cases in which the tentative diagnosis of tubercle had not been specifically stated—although probably suspected—I felt justified in classing as chronic pulmonary tuberculosis; in two of these the sputum was positive. The others were of the old-standing type, with chronic bronchitis and emphysema, several of which had been admitted with a diagnosis of asthma. In dealing with these cases I find that one of our most valuable elements in framing a diagnosis is absent. I refer to the history of the illness, on which most of us look for a third of our evidence. I have yet to find the pensioner who is willing to give a clear statement as to symptoms of illness before he joined the army! I am glad to hear Professor Rist insisting on the frequent examination of the sputum. One would have expected to find this elementary point had long ago been clearly grasped, but in my experience this is not so. I have had cases in the army and among pensioners where the value of arduous search has been amply proved. In one case tubercle bacilli were found on the thirtieth consecutive examination, in another on the twenty-eighth, in another on the eighteenth, in another on the fourteenth, and so on. Tubercle bacilli at times come in showers, and these are liable to be missed. Laureston-Brown's remark on this subject is worthy of citation: "Failure to find and interpret aright physical signs may be excused, but failure to ask for and examine sputum cannot be condoned." It is unfortunately difficult in this country to make free use of guinea-pig inoculation in diagnosis. The next question I found of interest in dealing with this group of cases is the reasons why cases are suspected of being tuberculous. They divide themselves roughly into three main groups as regards these pensioners, as follows: *Group 1*, Cases showing well-marked local pulmonary symptoms, with bronchial physical signs in excess of pleural or lung tissue signs. *Group 2*, Cases showing well-marked chest physical signs of a local nature, the symptoms (local or general) being not unduly prominent. *Group 3*, Haemorrhage cases—that is, patients who state that they have expectorated blood.

In the first group one finds "gassed" cases, or, more strictly, pensioners who state that they have been gassed and show symptoms and signs of chronic bronchitis and emphysema in varying degree. At times such cases are rather a puzzle, for various reasons. In the first place, the history is of practically no value, and one has found little help from medical literature in relation to the late effects of gas poisoning. Secondly, we know that warfare gas caused the lighting up of some cases of tuberculosis, but, as French and American authorities have pointed out, not nearly so frequently as one might at first suppose. Thirdly, *x* rays have demonstrated a peribronchial fibrosis with hilus enlargement in many "gassed" cases, and the plate or report is frequently returned with the label "peribronchial tuberculosis." Fourthly, as a consequence of gassing, cough is induced, giving rise to further development of chronic bronchitis and emphysema, a lesion commonly associated with a fundamental tuberculous process. A short paper by John B. Hawes of Boston points out how gassing induces irritative and escharotic effects on the respiratory tract, and the healing process is found to induce scar tissue, contraction, and mechanical defects. This naturally leads to a proliferative fibrosis which destroys the natural protective properties of the bronchi and leads to secondary infection, in which he says tuberculosis plays a minor part. In these cases shortness of breath and nervous phenomena are prominent features. Many such cases are classified as D.A.H. The prolonged duration of symptoms in the absence of toxic phenomena go far to establishing a non-tuberculous diagnosis in these cases. The patient, not his lungs, requires treatment.

In the second group one finds two subdivisions. (*a*) Gunshot wounds of the chest, where there has been resulting deformity and fibrosis. It is common knowledge that there have been fewer cases of active tuberculosis following wounds of the chest than one might have supposed. I have only had three cases in which tubercle bacilli were isolated from the sputum. Most of the gunshot wound cases have lacked the element of systemic intoxication, and only showed mild local symptoms despite well-defined chronic physical signs of a localized type. (*b*) This subdivision includes the old bogy of failure to appreciate the normal exaggeration of physical signs at the right apical region. That this is still a trap for the unwary we have on the excellent authority of Sir James Kingston Fowler, who says in his recent book on pulmonary tuberculosis: "We desire to mention that we have been in the habit of revealing to students the fact that a modest competency may be realized by correcting the errors of those who find the right apex seriously diseased when it is quite normal."

In the third group I find a few cases giving a history of haemoptysis which at times is misleading. Some patients in this group are distorters of the truth, and others interpret a mere streaking of sputum with blood as a frank haemorrhage. Careful cross-questioning as to the type and duration of the haemorrhage, also its relation to severity of coughing, usually elicits sufficient information to estimate this symptom at its true valuation.

The differential diagnosis of pulmonary disease is essentially the field for the skilled physician who can sift all the evidence available from physical examination, *x* ray findings, bacteriologists' findings, etc. Apart from tubercle bacilli in the sputum, there is no certain sign of pulmonary tuberculosis, and it is only by the correct valuation of all points that reasonable accuracy can be obtained. We should be able to diagnose our cases, and not merely say such a one is not tuberculous.

Dr. JAMES LAWSON (Tor-na-Dee) said: From the point of view of a doctor dealing entirely with patients who are already in a sanatorium the question of the differential diagnosis of pulmonary tuberculosis becomes somewhat limited, but there must always be, I venture to think, in every sanatorium a number of cases in which, although there is evidence to lead to the suspicion of tuberculosis, a definite diagnosis has not been made. In an endeavour to reduce the number of such "uncertain" cases I wish to direct your attention for a moment to a condition first described by Castellani in 1905, and which has not received adequate recognition and investigation in this country. I refer to the condition known as broncho-pulmonary spirochaetosis. Cases have been described in Switzerland, France, and Italy, but so far as I have been able to trace them only four cases have been published as occurring in this country, and of these two were without possible connexion with the tropics. To these

I might add a case at present under our own observation at Tor-na-Dee Sanatorium—a lady, who has never been out of this country.

As described by Castellani, there appear to be two types of disease—an acute and a chronic form; both are liable to be mistaken for pulmonary tuberculosis, but it is especially to the chronic form that I would direct your attention, as all the cases so far described in this country fall into this category. Males appear to be more frequently affected than females, and the age incidence tends to be over 30. There may be a history of a febrile attack at the commencement of the illness, but for the most part cases are afebrile. If fever is present, Castellani considers it more usual that it should be of the inverse type. Cough with expectoration is present and haemoptysis has occurred in every case as yet described—this may vary from mere staining of the sputum to a frank haemoptysis. The physical signs in the lungs may be negative, or more usually those of bronchitis, but consolidation may also be present. The general condition of the patient is rarely affected to any extent. The sputum cannot be shown to contain tubercle bacilli, but always swarms with spirochaetes whose morphological characteristics, as Castellani pointed out, vary widely even in the same case. The clinical picture is one which could easily be accepted for pulmonary tuberculosis, but the cases tend to diverge from the ordinary case of pulmonary tuberculosis in the following ways: (1) Absence of tubercle bacilli in the sputum. In the case which we have under observation no tubercle bacilli have been found, although each twenty-four hours' sputum for the past two months has been subjected to the concentration method of examination. (2) Enormous numbers of spirochaetes are always present in the sputum. (3) There is an almost entire absence of systemic disturbance. *X*-ray photographs do not show the usual shadows found at apices and further reveal no positive evidences. (4) In the case we have under observation the sputum presents certain peculiarities. It is thin and watery, containing greyish-green mucoid material, which has been likened to gooseberry juice. But the most characteristic feature is a peculiarly nauseating, penetrating, acrid odour, which becomes greatly intensified when the sputum has been allowed to stand for some time. The odour is quite distinctive. I know of nothing which it resembles.

The question which naturally arises at once in this connexion is whether this condition of broncho-pulmonary spirochaetosis is a pathological entity or whether it is merely a superadded spirochaetal infection to a tuberculous lesion. The absence of the tubercle bacillus does not negative the diagnosis of tuberculosis, and the presence of the spirochaete in tuberculous sputum is well known, but in these cases its probable origin is in oral lesions. Most of the cases of broncho-pulmonary spirochaetosis show oral lesions, but in the case under our care, even after efficient dental treatment and treatment of the gums, the sputum, collected with all possible precautions, showed no diminution in the number of spirochaetes. So far as I know, only one *post-mortem* examination has been reported with the idea of distinguishing any pathological change in the lung as the result of a spirochaetal infection, but as the patient also suffered from pulmonary tuberculosis, syphilis, and malaria no conclusion can, I think, be drawn from the findings. I feel that there can be no doubt that broncho-pulmonary spirochaetosis is clamouring for recognition as a separate disease, although further investigation is necessary to settle the question definitely. I hesitate to suggest what proportion of undiagnosed or wrongly diagnosed cases should be included under the label "spirochaetosis," and, although not large, I think they would be appreciable. As prognostic, therapeutic, and administrative considerations rest on the recognition of the condition, I consider that it should be given a more conspicuous place in the differential diagnosis of pulmonary tuberculosis.

Dr. C. MUTHU (Mendip Hills Sanatorium) said: The differential diagnosis of pulmonary tuberculosis presents many difficulties even to expert observers, because it has a wide relation to many diseased conditions, its symptoms and signs closely resemble those of many other respiratory affections, and because its physical evidence varies in different individuals, there being no recognized standard of physical signs common to all cases of pulmonary tuberculosis. While it is true, as Professor Rist has pointed out, that many cases are wrongly diagnosed as pulmonary tuberculosis, it is also much more true that a positive diagnosis is more often missed than wrongly made. I quite agree with Dr. Pottinger, of America, when he said that we are missing tuberculosis ten times more often than we are diagnosing other diseases as tuberculosis. One reason is that a large number of phthisical cases do not go farther than the incipient stage and get well of their fever, cough, sputum, etc., after a change and rest in the country, leaving behind a certain amount of impaired resonance and harsh breathing. In the diagnosis of pulmonary tuberculosis we come to deal with three sets of evidence—the presence of tubercle bacilli, physical signs, and symptoms. When tubercle bacilli are present in the sputum the diagnosis is positive in many cases. But we have this curious fact that in some cases tubercle bacilli may be found in the sputum and yet the patient may not be tuberculous. This may be due to the difficulty of differentiating between tubercle bacilli and the acid-fast rods getting into the sputum from milk, butter, the soil, manure, saliva, and even from water. On the other hand negative sputum does not indicate the absence of tuberculosis. In many cases with well-recognized signs and symptoms of phthisis tubercle bacilli cannot be demonstrated even after careful and repeated examination. Further, as tubercle bacilli make their appearance only after the lung breaks down their evidence does not help in the diagnosis of early cases or in those cases which remain in the incipient stage for months and even years. So more important than the presence of tubercle bacilli is the evidence of physical signs, especially auscultation. A wavy, jerky, and harsh breathing, crackling or crepitant râles heard at the end of inspiration, especially following a cough and confined to the apex or apices, ought to make the diagnosis certain. The more we depend upon laboratory evidence the more auscultation is apt to be neglected and the more it fails to help in the diagnosis. Only life can interpret life. The living ear of the physician can detect and interpret correctly the various shades of breath sounds when the dead physical helps are unable to give a decided diagnosis.

Sometimes even physical signs may fail. I have seen cases, especially in India, where nothing but a high temperature and rapid pulse and sometimes enlarged glands mark the course of pulmonary tuberculosis for many weeks with vague or no physical signs. These cases may take an acute galloping form or end fatally from general toxaemia. I call attention to this Indian type as it is apt to be mistaken for malarial and other fevers. Also in some chronic and afebrile cases physical signs may be very obscure. So more important than physical signs are the evidence of symptoms, such as a slight but persistent rise in temperature, rapid pulse, mild but constant cough, loss of weight, languor and tired feeling, anaemia, dyspnoea on exertion, anorexia, symptoms of dyspepsia, slight haemoptysis, etc. If these characteristic symptoms, when present, are carefully studied and valued with due regard to perspective, they can be relied on to yield a fairly accurate diagnosis of early tuberculosis even if tubercle bacilli are not found in the sputum. It is possible that almost all these symptoms, as Professor Rist and others have shown, may be shared by many respiratory diseases such as acute bronchitis, bronchiectasis, subacute lobar pneumonia, acute colds and influenza, apical catarrh, etc. Most non-tuberculous cases occur in the middle or lower lobe of the lung, and constitutional symptoms, such as temperature, quick pulse, loss of weight and strength, are not so severe or progressive as in tuberculosis. But differential diagnosis becomes very difficult in apical and localized bronchitis and bronchiectasis, in unresolved pneumonia where acute symptoms continue more or less indefinitely, in influenzal pneumonia, and other pulmonary coccal infections. Many a case of so-called influenza is but an acute attack of latent tuberculosis; and bronchopneumonia and lobar pneumonia may be the beginning of acute pulmonary tuberculosis. In one of my sanatorium cases pneumococci were present in the sputum for several months and were gradually replaced by tubercle bacilli. These cases, presenting almost all the signs and symptoms of phthisis, may be the forerunners if not identical with pulmonary tuberculosis. Especially, apical catarrh and collapse induration of the apex in strumous children brought up in poor surroundings and insufficient food, cases which Bard and Fishberg call "abortive tuberculosis," cases of hyperthyroidism with lung complication, in all of which well-marked signs and symptoms (such as fever, rapid pulse, dullness of apex, impaired movement, dry and moist râles, cough and expectoration, and even slight haemoptysis) very closely simulate those of early phthisis, should to my mind be classified as incipient or benign form of tuberculosis. In fact, I plead that our ideas

and interpretation of pulmonary tuberculosis should be widened and not be confined only to cases which present a positive evidence of tubercle bacilli, and that all cases where differential diagnosis is difficult and impossible, requiring time and observation, should be sent to sanatoriums which should be observation centres and for early treatment. In this way we give the patient the benefit of the doubt and ensure speedy recovery, and thus save time, trouble, and expense both to his family and to the State. I cannot help suspecting that at least some of the so-called non-tuberculous cases—of course excluding all other possible causes—are obscure and ill-defined forms of tuberculosis or cases that stop at the incipient stage and get well after a few weeks' treatment. The family physician is faced with this dilemma: if he sends early cases for treatment before tubercle bacilli appear in the sputum he may be told that it is a non-tuberculous case; and if he waits for the appearance of the bacilli he risks the patient's life and makes the chances of his recovery uncertain. But the fact already mentioned, that more cases of phthisis are overlooked than mistaken, should help to sink the scale for early diagnosis and treatment, even though tubercle bacilli cannot be demonstrated in the sputum.

Dr. A. HOPE GOSSE (London) confined his remarks to the evidence on which Dr. A. Lisle Punch and he claimed that, with a method (described by his colleague) of doing the complement fixation test, they had an almost infallible guide as to the presence or not of active tuberculosis. The blood of the patients with tubercle bacilli in their sputa were positive in 98 per cent. of cases. Control tests, with serums from cases without clinical evidence of active tuberculosis, were negative in 98 per cent. Fifty Brompton Hospital patients, suspected of the disease until the test was done and found to be negative, were still free from any development attributed to tuberculosis in 49 of the cases some ten to twenty months later. Likewise in a series of 50 consecutive suspicious cases, in which tubercle bacilli had never been found when they gave a positive test, of those able to attend between a year and two years afterwards 45 per cent. had tubercle bacilli in their sputa. With the assistance of this test a confident diagnosis of pulmonary tuberculosis had been made in one of these cases two years before tubercle bacilli had been found in the sputum. Their experience showed that the test was almost always positive by the time the symptoms due to the infection had developed. The blood still gave a positive test for perhaps about a year after the disease was arrested. It had been authoritatively stated that early diagnosis was the most important factor in the prognosis of the disease, and it was therefore with a full sense of their responsibility that they announced that in the test, performed by the method referred to, there was now an almost infallible guide as to the presence or absence of active tuberculosis.

Dr. SINCLAIR GILLIES (Lecturer on Clinical Medicine, University of Sydney) thought that the tendency of the meeting was to rely too much on physical signs and tests. What, he asked, was the distinction between "infection" and "disease"? Was it not evidence of toxaemia, and did not diagnosis depend on correlating physical signs and tests with such evidence rather than on placing reliance on any one test or sign?

Dr. EDWARD PREST (Ayrshire Sanatorium) said that they must remember that medical superintendents of sanatoriums spent their time considering for what length of time patients were kept from any kind of effective treatment. Diagnosis was largely a question of symptoms. *X* rays would not tell whether the disease was tuberculosis, and whether it was active or not. To wait for a positive sputum might be to wait too long. In times of war it was often difficult to rely altogether on a patient's own statements regarding the state of his health. The condition must be diagnosed from symptoms, and in a suspected case other diseases must be excluded, and appropriate treatment then applied. If patients had no symptoms, it would not be necessary to give treatment, but if symptoms were present they should be treated at once. Physicians who were called upon to diagnose and report on suspected cases of tuberculosis should always endeavour to give some explanation of what "negative" patients were suffering from.

Dr. A. S. M. MACGREGOR (Glasgow) referred to the *x*-ray demonstration which had been arranged by Dr. Henderson and himself in connexion with the discussion on diagnosis. In this demonstration a set of one hundred films illustrative of different lesions in the lung were shown. From the point of view of difficulties, both in clinical and radiological diagnosis, a special inquiry had been going on in Glasgow in connexion with the large group of cases of fibrotic lesions in the lung, and the demonstration showed instances of peribronchial fibrosis following pneumonia and other acute or subacute lesions of the lung, and also silicosis and other affections.

[A series of films was also shown illustrating the difficulty of determining what is to be regarded as a normal *x*-ray photograph.]

THE SHARE OF THE SUN IN THE PREVENTION AND TREATMENT OF TUBERCULOSIS.

BY

A. ROLLIER, M.D.,

Leysin, Switzerland.

I WISH in the first place to express my thanks to your President, Sir Robert Philip, for the honour he has done me in asking me to come to your meeting in Glasgow. I have come here as the representative of a belief to which I have devoted my whole life, and which is that the sun is the source of life not only for the healthy but also for the sick, and that it should be expected to become an important weapon in the therapeutical arsenal against tuberculosis.

I am particularly happy to give this demonstration under the patronage of such a well-known authority as that of Sir Robert Philip, who was one of the first to organize the preventive measures against tuberculosis in this country. It is a great pleasure for me to know that in your country the value of light and sun has been officially recognized, so that a committee on the action of sunlight has been appointed lately for a scientific study of this question including such eminent men as Sir William Bayliss and Professor Leonard Hill. Let me remind you that this committee has been appointed owing to Dr. Saleeby's very courageous and disinterested campaign for a better hygiene of the towns, the suppression of smoke, and the triumph of light. I am also well aware that sun and light not only are studied in your country, but that heliotherapy has been applied for many years and with well-known success by men like Sir Henry Gauvain in the admirable institution at Alton, by Dr. Pugh at Carshalton, and perhaps by many others who may be here and whom I do not know.

The awakening interest in heliotherapy throughout all medical circles in the world is not only due to the general recognition of the value of a method based upon a more accurate apprehension of the nature of tuberculosis, but perhaps more certainly on account of its special value, the fight against tuberculosis being primarily a social question. The terrible havoc wrought recently in almost every country by tuberculosis has aroused a more widespread interest in this burning question. All that tends to the elimination of this scourge is of supreme importance. Internal and external tuberculosis are not only local ailments requiring local treatment, they must be regarded as being the seat of a general infection. In dealing with such a general infection, showing local symptoms and appearing in local seats of disease, a therapy giving a general effect must be employed in order to restore the normal constitutional condition of the individual, together with a local treatment which, to be rational, must never and in no way interfere with the necessary rebuilding of the whole system. It was to this end that, in 1903, we opened the first clinic for air-cure combined with systematic heliotherapy at a high altitude, in Leysin, a village of the Swiss Alps (4,500 feet above sea-level).

Open-air life, so eminently reconstructive, associated with complete sun baths, such as we have advocated it since the beginning of our work—that is to say, the direct action of the sun over the entire surface of the teguments—constitutes the most energetic of excitants and tonics. At the same time heliotherapy realizes the rational local treatment, adding to its reconstructive action on the body the analgesic, bactericidal, sclerogenous, and reductive effect of the solar radiations. Sun cure may be carried out wherever the sun shines—on sea-level, by the seaside, on the mountain. If we have chosen the high mountain in which to erect our clinics it is because the alpine air and sun bath may be there taken during each month of the year, thus permitting uninterrupted cure, maintaining the body at its maximum vitality and power of defence.

Action of Solar Rays.

How does the sun act upon the organism? That is one of the most captivating questions, a clear statement of which could be but imperfectly outlined even in a long lecture. Our limited time only allows us to take a very rapid survey of this part of our subject. We should first remember the double action of light on the organism—its direct and indirect action. The direct action has been doubted for long, because the bactericidal violet and ultra-violet rays have only a superficial action. Wiesner's work has, however, proved that the infra-red rays, especially those nearer the red, are also bactericidal, and by their long wave-length can penetrate the organism and thus might have an action on deep-seated lesions. But if the direct action of the sun is still matter for discussion, the capital part it plays indirectly has been proved by numerous experiences which we can only sum up very briefly. Everyone knows that the violet and ultra-violet rays, detained at a depth of one millimetre in the skin, are absorbed by the superficial sanguine surface formed by the net of the capillaries now dilated by the sun. To these rays must be attributed the phenomena of erythema and pigmentation.

Pigment is not only a protective agent against too violent an irritation by ultra-violet rays, but it also regularizes the thermic share of the sun. The profound biological value of pigment is everywhere better recognized. It is very likely that pigment receives, supplies, and increases the activity of the elements essential to the metabolism of the hormones. You know that Bloch has demonstrated that a substance, chemically very much like adrenaline, submitted to the action of oxidized ferments of the epiderm produces the formation of normal epidermic pigments, light strongly accentuating this formation. On the other hand, the skin's ferments seem to depend upon the quantity of vitamin contained in the food, as has been recently proved by the experiment made in Vienna on rickety and insufficiently fed children. Let us also remember that recent German and American publications have demonstrated that the action of radiations produce a very strong retention—which can reach as far as 400 per cent.—of the phosphates and lime in the organism. Let us remember, finally, that Naegeli has proved the action of sun on the haematopoietic system to be followed by an increase in haemoglobin, that Behring has ascertained the more active respiration of the red globules submitted to the sun, and that Schläpfer has given the proof of the photo-activity of a photograph plate, by means of previously insolated blood. Lastly, we might mention the recent experiences of Sonne in Copenhagen, who has proved that under the action of radiations the temperature of the blood, at a depth of more than one-third of an inch in the skin, augments to about 5° C.

From these experiences must be drawn the conclusions that the light absorbed by blood changes it into a receptacle of radiant energy. Transferred into the whole organism this energy hastens the intracellular processes of oxidation and reduction, thus modifying the whole metabolism. Consequently in this way the defence of the organism exposed to the beneficent action of the sun is progressively increased, making it better fit for the fight against tuberculous infection.

The Skin.

Among the general effects of the sun on the body we would first mention its action on the skin. My reason is that we attribute in our results a capital importance to the remaking of this essential organ through the working of the sun. The skin is not only the natural clothing of the body, wonderfully fitted to protect it against the penetration of pathogenic organisms. Provided as it is with a compact web of capillary vessels, it forms a real expansion of the vascular system. These vessels, by their contraction and dilatation, constitute a kind of "peripheric heart"—an admirable regulator of the circulation of the blood. The skin is also an expansion of the nervous system; this close-woven net of sensory and sensitive nerves may be compared to a keyboard, on which all exterior impressions provoke resonances in the whole organism. The skin is not only an organ of protection, circulation, innervation, but also one of respiration and nutrition, since it gives off the carbonic acid and water vapour and absorbs oxygen. Finally, the skin constitutes also an essential organ of elimination; for this reason it has been called the "vicar of the kidney"—perhaps even more important than his superior, for one can easily live having one kidney left, whereas it suffices that one-third of the cutaneous surface should be destroyed through burns to cause death.

These important physiological functions can only be accomplished if the skin is placed in its natural surroundings, and its natural surroundings are the permanent contact with air and light, just as water is the natural element for the scale of a fish. Weakened and atrophied for many generations through the wearing of unhygienic clothing, the skin, under the action of air and sun, recovers its many and vital functions, and becomes the ideal clothing foreseen by Nature. Gradual formation of pigmentation on the teguments gives to the body a progressive resistance against cold and heat, opposes infection of sores and wounds and shares in their cicatrization. Pigmented teguments are refractory to numerous cutaneous affections, such as furuncles, acne, etc., which often are merely doors through which more serious diseases may force an entrance.

The sun bath, enlarging the capillaries, determines an afflux of blood towards the skin; this ameliorated and regulated circulation of the blood helps to make the musculature remarkably firmer, better than the best massage would make it; thus the body findsagain its natural harmony. The tonic power of the sun manifests itself also on the thoracic and abdominal organs; under its influence haemoglobin increases, internal secretions accentuate, digestive functions become regular, weight increases, strength is renewed. The sun not only acts on the physical side, it has a most efficient effect on the mind and the character of the patients. This is of great importance when one remembers how much this influence reacts on the physical, especially with patients who have to remain a long time confined to bed.

Posology.

How must sun treatment be dosed? This is the aim of posology, which is one of the most important chapters in sun treatment; time does not permit of my dwelling on it as it deserves and as I should like to do. On knowledge or lack of knowledge of posology depends the result that we may expect from sun cure, and let us bear in mind that no physiotherapeutic method demands such strict individualization as heliotherapy.

A very current mistake consists in thinking that the sun bath is all the more efficacious if prolonged or taken when the sun is at its hottest; this is an inconsistency against which Nature herself seems to warn us.

We have laid down very precise rules for the practice of sun cure in order to prevent miscalculations and accidents. The principal rule is this: one must begin by only exposing the lower less sensitive parts of the body to the sun's rays. This will serve to show in what measure the patient bears the sun bath, and his further progress in insolation will be based on such observations. This method of derivation avoids congestion of the head and of the thoracic viscera. One must not take air and sun baths during meridian hours, especially in places at sea-level, where the ambient atmosphere enfeebles, produces congestion, annihilating the good effects of the solar rays. During the warm months of the year it should therefore be taken in the early hours of the morning.

Air and sun baths should always be taken gradually, the duration of insolation increasing by a few minutes each day, first exposing the feet, then the legs, then the thighs, the abdomen, the thorax, keeping the head covered with a simple linen hat. After a certain period of cautious advance, varying according to the case, the season of the year, the heat of the sun, the body becomes pigmented and can then bear several hours of sun each day. As a general rule, the sun bath, to be beneficial, should create a sensation of profound well-being, and each patient, whilst taking it and afterwards, should find himself invigorated.

Technique.

Before speaking of our technique, I think I ought to give you some idea of the installations in which we practise heliotherapy. I have neither the time necessary, nor the desire, to describe our thirty-four institutions in detail, but I would like you to see the first house we opened. It was then the only little boarding-house in our village; it is now one of our free clinics, opened in 1903. We fitted it up for the sun cure, by the addition of large balconies and of a roof solarium which probably was the first one to be built in Europe since the time of the Romans. Our contention is proved that heliotherapy can be successfully applied otherwise than in ruinously expensive palaces, for it is in this simple chalet that we obtained most of the cures to be subsequently shown and described.

It is indispensable to say a few words as to what has been the teaching of experience in regard to the application of the

sun cure. We will take the different localizations in their turn, beginning with Pott's disease and the most important articulations.

The Bed.

As the bed plays a very essential part in the treatment of such localizations, and is, so to speak, one of our principal tools, I take the liberty of insisting on some of its particularities. Its indispensable features are the following: It must be high, so that the balcony rails beside it may not obstruct the sun or the view, and to facilitate the nurse's work. It is fitted with large, smooth, easily running wheels, and can be rolled on to the balcony, to concerts, and even to the garden.

Special very hard mattresses are used. These never lose their shape, give the patient's body a maximum of support, do not induce perspiration, and prevent bed-sores. The spring mattress is composed of widely spaced strips of metal, thus ensuring perfect aeration of the mattress. Very emaciated patients are padded in with cushions.

Pott's Disease.

From the beginning we have discarded plaster appliances in cases of Pott's disease, finding that they bring about atrophy not only of the skin and muscles but of the skeleton itself. These cumbersome plaster devices have been replaced by simple arrangements of webbing straps which give sufficient immobilization while permitting the free access of air and sun to the injured places. Patients, especially children, are immobilized by a simple apparatus consisting of linen stays or braces, attached by means of linen straps to the head and foot of the bed itself. These can be opened and closed at will and cannot in any way interfere with the sun and air cure. Straps are also fastened down over the knees to keep them in position.

As soon as an *x*-ray examination shows the beginning of calcification of the vertebral block, the patient is placed in the ventral position, the thorax supported on a triangular cushion on which the patients can lean their elbows. To this physiological position *par excellence* we attribute a very great importance. This same treatment, as regards position, was advocated about a hundred years ago by two English doctors, Banting and Bamfield, and had been long abandoned when we again gave it the consideration it deserves. This position augments the lordosis and thus tends to correct the pathological curvature. It permits the insolation of the whole dorsal region and so very considerably develops and strengthens the muscles and ligaments.

Once cured, our cases of Pott's disease are all provided with what we call the "muscular corset," which is the best support of the spine for the future. In certain cases, in order to help to the correction of kyphosis by straightening of the shoulders, we use braces which are fastened to the foot of the bed.

[Dr. Rollier then showed a number of photographs of patients.]

1. A patient suffering from Pott's disease, with bad general condition, a very extended and prominent kyphosis, cured and straightened in quite a satisfactory manner by the proceeding just explained, after two years' treatment.

2. A young child suffering from extremely painful Pott's disease with marked curvature, cured after eighteen months' treatment. The remarkable carriage of the child, his well-developed muscles, and the happy expression of his face were evident.

3. Another case of Pott's disease with very prominent kyphosis and marked atrophy of the thorax, both caused by plaster apparatus applied during many years. The patient was absolutely transformed after two years' sun cure, having recovered his muscles, and looking like an athlete.

4A. A case of Pott's disease complicated by mixed infection, with a sinus in the region of upper third of thigh, also with prominent curvature. The patient's muscles had been atrophied by wearing plaster apparatus for many years. He was also cured and partially straightened after twenty months' sun treatment.

4B. A picture showing the same patient in the dorsal position, lying on cushions, one of them replaced by a wooden block. The patient was first accustomed progressively to the wood by cushions filled with millet, then with sand. The advantage of the wooden block is that it offers a very uniform resistance and cannot hurt the skin, as such a block is always very smooth and dry, thus never producing scabs. It is generally very well tolerated by the patients, many of them preferring it to any other cushion.

4C. The same patient in ventral position, busy typewriting. To all of the patients is advised the work cure, which is applied systematically in most of our clinics.

5. Another case of Pott's disease, practising insolation while doing basketwork which had been taught him in the free clinic where he is treated. This work allows him to pay part of his board and gives him a happy feeling of independence.

6. A balcony where this work cure is applied. On the foreground was a case of Pott's disease practising insolation on his back while doing basketwork.

This prompts me to say a few words on the work cure, which was first introduced into England by Dr. Paterson at Frimley Sanatorium, in the case of patients suffering from pulmonary tuberculosis, and which we organized first in our institutions for surgical tuberculosis. I wish to insist on this most valuable factor in the victorious conflict against mental depression, and which we consider to be a most precious adjuvant to heliotherapy. We always advise its practice for all our patients.

I personally consider the part played by work cure so important at the same time from a therapeutical, physical, and mental standpoint that I wish to show how we practise it in several of our institutions, by a film. It will give you an idea of how we had realized the work cure in one of our military clinics during the war.

[Dr. Rollier then showed a cinematographic film illustrating the work cure.]

Hip Disease.

For hip disease we use practically the same appliances as for Pott's disease. Plaster apparatus has been discarded here also, and has been replaced by continual extension by means of a sort of double bracelet made of linen or leather, fastening round the knee and ankle, which can be taken off at will. It always allows the insolation of the hip, and even of the whole leg. We also use a small sandal in order to avoid the tendency to equinism.

[Dr. Rollier again showed photographs illustrating cases.]

7A. A case which had been cured by this method, a very advanced case of hip disease, bad position, considerable shortening, partial luxation, external rotation, who arrived in this condition in a plaster apparatus.

7B. The same patient after eighteen months, quite cured by continual extension and sun cure. There was very satisfactory correction of the shortening, the muscles were developed, there was restoration of the whole organism, and with quite a satisfactory partial return of the articular function.

8A. A very severe case of hip disease, who arrived in a miserable condition, complicated with numerous sinuses of mixed infection. With all cases of this kind a good drainage of the sinuses is first assured, so as to avoid retention of pus, intoxication, and fever. Only when the drainage has been obtained is insolation practised in the region of the sinuses.

8B. The same patient after two years, cured, all the sinuses dried, restoration of the whole general health, and partial but quite satisfactory return of function.

I wish to add that with cases of hip disease with bad attitude the so-called bracelets which I have mentioned are not always sufficient for extension, and that a more complete appliance is needed. We then apply extension by means of bands of sparadrap. To make the extension more easy the leg is placed on a splint provided with wheels and placed on a small cart, the rolling of which is extremely soft, and which allows an energetic extension to be got with little weight.

9. A very serious case of hip disease with partial luxation of the head of the femur, very important shortening, bad position in flexion, severe secondary infection. The system of extension was demonstrated: the cushion which elevated the pelvis was to be noted, thus literally presenting that part to the sun's rays. It helped to avoid the bad attitudes and to correct them.

Tuberculosis of Knee.

In cases of tuberculosis of the knee we also have left plaster appliances. When the knee is painful it is placed in a widely opened splint, with continual extension. The injured limb is always placed on an inclined plane, which makes regression of capsule and periarticular infiltrations much more easy. The plane is more or less inclined according to the gravity of the case.

10A. A case treated after this method. He arrived in Leysin after resection of a tuberculous knee in a desperate condition. He had numerous wounds, not only in the region of the knee, but on the whole thigh. These wounds were consecutive to abscesses which had been incised and infected. There was bad position of the leg, ankylosed in internal rotation, very bad general health. The patient was feverish, cachectic, and suffered acute pain.

10B. The same patient after two years. The knee was cured, in ankylosis, of course, owing to the resection. There was cicatrization of all the wounds and the general condition was splendid. The young man was actually, and had been for ten years, at the head of a rural settlement, and always in perfect health.

11A. A young girl with tuberculosis of the knee; focus in the external condyle, beginning of periarticular infiltration, muscular atrophy, bad general state.

11B. The same patient after one year and a half; the knee was perfectly cured, outlines quite normal, and complete articular function, while the general health was absolutely restored.

12A. A case of very advanced tuberculosis; this patient was condemned to amputation. There was arthritis of the knee, very advanced, with subluxation, complicated with mixed infection, and a very bad general condition.

12B. The same patient, his knee quite cured, luxation nearly corrected, and his general condition splendidly restored. His expression confirmed this happy result.

I must be excused for repeatedly insisting on the restoration of the physical and the moral conditions, which are, as you are well aware, capital points in the question of tuberculosis.

Tuberculosis of Feet.

In the cases of tuberculosis of the feet we also use immobilization on the inclined plane in little splints which are widely hollowed out, or a mobile sandal allowing the progressive straightening when equinism, as often is the case, accompanies tibio-tarsal arthritis.

13A. A patient suffering from recurrent tuberculosis of the feet who had been operated on several times; she also suffered from tuberculosis of both elbows, with a miserable state of health. Her face wears an expression of pain and discouragement.

13B. The same patient again after two years, absolutely cured of every manifestation of tuberculosis. The beauty and harmony of her body was to be noted.

14. A case of tuberculosis of the foot who had been condemned to amputation—absolutely cured after fifteen months' heliotherapy.

Peritonitis.

Heliotherapy gives the most favourable results in cases of peritonitis, but this is, of all the localizations, that which requires the most careful supervision, especially as it is often accompanied by pulmonary and pleural complications. It is quite natural that the peritoneum, which presents a great surface vascularization, should become very easily congested, and this is where our method, in French described as the "derivative method," proves itself of singular utility. It provides for a slow and progressive insolation which draws the blood down to the extremities, thus obviating the possibility of such congestion as might follow sudden local exposure to the sun.

15A. A severe case, a young girl suffering from extremely advanced peritonitis, ascites, and pleural exudate as well, very bad general condition, considerable atrophy of skin and muscles.

15B. The same girl, completely cured after one year. On comparing the condition of the muscles it was astonishing to note the sculptural reconstitution of this body, which was reached by sun cure only, without the addition of massage.

Ileo-caecal Tuberculosis.

Heliotherapy gives quite as favourable results in cases of fibro-caseous and fibro-adhesive peritonitis, as well as in all forms of ileo-caecal tuberculosis. The advantage this treatment has over operative measures is that such complications as formation of faecal fistulae, which are rather frequent after resection, can be avoided.

16. A young patient who had been operated on several times for ileo-caecal and peritoneal tuberculosis; these interventions had been followed by the formation of five faecal fistulae, which were seen on the picture. These were all closed, while at the same time every manifestation of ileo-caecal and peritoneal tuberculosis disappeared, after eight months of heliotherapy.

Adenitis.

Heliotherapy is also the treatment of choice for cervical adenitis, which is always cured without leaving any scars, according to three different processes: "melting," generally followed by aspiration; spontaneous resorption; or sclerosis. The two first modes are the most frequent. Only exceptionally do we have recourse, in certain cases of adenitis with sclerosis, to the association of *x*-ray therapy to heliotherapy. The exclusive use of heliotherapy has the great advantage of never giving rise to such accidents as radiodermatitis.

17. A young patient with adenitis who was quite cured by melting and aspiration after six months' heliotherapy.

18. A young girl suffering from open adenitis with formation of very extended wounds with granulations, and who was quite cured, with scarcely visible scars, after eight months' sun cure.

19. A particularly interesting case of secondary adenitis consecutive to a primary infection of the conjunctiva. This happened after the patient, who was a young doctor, had had his conjunctiva infected while carrying out an autopsy. Everyone is aware that extreme reserve must be practised in the prognosis of such cases of primary tuberculosis of the conjunctiva. This young colleague had tried all the usual treatments: cauterization had been attempted, then excision of the palpebral conjunctiva—all without success. Complete cure was obtained, with that of the secondary glands, after seven months of heliotherapy. Since that time the patient was at the front during the whole duration of the war, and was at present very actively employed in France, having always kept in excellent health.

Lupus.

Heliotherapy gives the most satisfactory results in all forms of lupus.

20. A severe case. The lupus extended not only to the external parts of the skin, but to all the mucous membrane of the nose and palate. All these manifestations disappeared under the effect of sun cure alone, applied during one year to the membrane and skin. It is very seldom that it is necessary to supplement heliotherapy by phototherapy or *x*-ray treatment.

The best orthopaedic and preservative surgery may be practised by means of heliotherapy; it safeguards the entirety of articular function to the utmost degree possible, and in so doing makes the nation a gift of citizens who are neither deformed nor incomplete, but of sound bodily capacity.

Furthermore, the treatment involved is not within the reach of wealthy patients only. Indeed, it may be argued that as a cure it is socially economical, since it is cheaper for a community to assure the complete fitness of one of its members, even though this may entail a year's, or perhaps even a two years', stay in bed (this time to be passed in the open air and sun), than that the inevitable mutilation attendant on an operation should lessen his value for ever.

Maintenance of Cure.

Clinical results obtained by sun cure are only of value if lasting. The best way for these who have suffered from surgical tuberculosis to maintain their cure is that they should henceforth live, as nearly as possibly, in conditions similar to those which determined the recovery. To avoid a relapse an ex-patient must neither take up an unhealthy occupation nor re-enter an unsanitary lodging, but rather choose a trade or profession that promises reasonably hygienic conditions of life.

To this end we have recommended and set on foot a market-gardening colony and an agricultural colony for our convalescent patients. The results, from the double standpoint of capacity for work and resistance of the constitution, have been decidedly favourable. Efforts should be made to extend these settlements, for it is of first importance to solve the great problem of the maintenance of the cure.

End-results.

It has often been said that the cures effected by heliotherapy are not lasting. We are now establishing records with a view to the publication of the statistics of early cures. This work is a very difficult one, most of our former patients having been foreigners, who are not easy to trace after the war, which has brought so many changes in all the countries. We had, however, the good fortune to receive news and photographs from our first cases—those that have been published previously—and we can now show a number of these patients, some ten, twelve, fourteen, and even more years after establishment of cure. This is all the more striking, judged as a result, because all these first patients were poor folk, or at least ill provided with this world's goods.

21A. A boy who had seventeen foci of tuberculosis, osteitis, periosteitis, adenitis. One lesion in the temporal region had been curetted. Secondary infection—sinuses, affection of tracheo-bronchial glands, and infiltration of right apex.

21B. Complete cure six months later.

21C. Thirteen and a half years after establishment of cure: wheelwright and agricultural labourer; did more than a year's military service without a day's illness.

22A. Extended tuberculosis of face and elbow. Seventeen sinuses. Numerous operations, one of them being a partial resection of the elbow. Secondary infection—ankylosis of elbow. Very bad general condition, high temperature, albuminuria.

22B. Complete cure after one year. Cicatrization of all the sinuses and complete return of articular function.

22C. The same boy; ten years later he was working in an office in Germany, having kept well in spite of the privations of the war.

23A. Tuberculosis of right elbow with formation of many wounds and sinuses. Secondary infection following several surgical interventions—very painful swollen ankylosis; very bad general health, high temperature.

23B. The same girl ten months later. Cure and cicatrization of all the sinuses.

23C. The same, showing return of articular function.

23D. The same, twelve years later—a strong cook, having kept in perfect health all this time.

24A. Tenosynovitis and tuberculous arthritis of wrist, recurring after operation. Secondary infection—sinuses, renal tuberculosis, nephrectomy, tuberculous peritonitis, tuberculosis of right apex,

small lupus on right cheek. General condition poor. Habitus phthisicus.

24B. The same patient one year later.

24C. The same patient, thirteen years after establishment of cure, in excellent health and very active as a nurse.

25A. A case of Pott's disease with large curvature; general muscular atrophy, advanced tuberculosis of right lung, with repeated haemoptyses. Very bad general condition. Habitus phthisicus.

25B. The same man after one and a half years.

25C. The same man at the head of an important grocery store twelve years later.

Pulmonary Tuberculosis.

It has often been said, and is still believed by many, that heliotherapy is dangerous and of no help in pulmonary tuberculosis. We are of a different opinion, and our experiences tend to prove our position. It is true that most of the cases which we have treated accompanied surgical lesions. By careful examination of the lungs of our surgical cases we discover that about 30 per cent. of them have pulmonary foci as well. We have, however, also had in our care a whole pavilion of Swiss soldiers with pulmonary tuberculosis only and have never met with an accident of congestion, and no soldier who has been treated by sun baths has ever presented manifestations of haemoptyses any more, even if subject to them previously.

So we are convinced that the sun is not an agent of congestion, but rather a derivative, and might be compared to a cupping glass, the action of which we can regularize. It must, however, be well understood that the sun treatment, in such cases, must be applied exactly according to our derivative method—that is to say, by always beginning by the exposure of the extremities, and proceeding by prudent and slow progression. The sun should be dispensed, so to speak, drop by drop, when nearing the region of the thorax. It is also essential, as we have always said, that the heat of the midday hours should be avoided for the treatment, and one should have recourse to the earliest morning sun, while the air is not yet overheated.

Needless to say, no patient with a high temperature or in a state of intoxication should be exposed to the sun.

26A. Tuberculosis of right knee, very painful arthritis with synovitis. Very great swelling, fixation in bad attitude, subluxation of tibia.

26B. The same, showing the return of articular function.

27A. Very bad tuberculosis of the foot, leg, and thigh; had been operated on five times. Thirteen sinuses, mixed infection, ankylosis of foot and knee. Very bad general condition, cachexia, albuminuria, high temperature.

27B. The same patient after one and a half years, absolutely cured, with complete restoration of the organism.

27C. The same ten years later, one of our best nurses.

28. Very extended and painful tuberculosis of the left foot, mixed infection, large fetid wound, abundant suppuration, bad general condition. Spontaneous elimination of necrosed scaphoid took place. This spontaneous elimination of sequestra is extremely frequent, and we only practise surgical intervention when the sequestrum is enclosed in a cavity from which it cannot make its way out. In all other cases we leave it to the care and foresight of conservative Nature, who knows much better than the surgeon what must be expelled and what kept. This patient was cured after ten months' heliotherapy. The wound completely cicatrized after the elimination of the sequestrum. Twelve years later the patient had remained perfectly healthy, and could engage in all kinds of sport.

29A. A case of Pott's disease; very pronounced kyphosis, paraplegia, atrophy of muscles.

29B. The same patient eighteen months later; cure of paraplegia, complete refection of general health and of muscles. After her cure this girl went back to Russia, where after a few years she went through the whole miserable experience of the Revolution. She had to escape to Siberia with her parents, living during three months in an abandoned railway carriage. At last she reached Shanghai, where she became overtired by working to earn her own and her family's living, and in the meantime studying hard, her heart's desire being to qualify as a doctor. Having saved a little money she left Shanghai alone and went to London, where she began her studies. But all her difficulties had told too much on her strength, and soon it was discovered that she was suffering from renal tuberculosis. She was operated on in London, then came back to Leysin, where her nephrectomy wound is actually on a good way towards cicatrization. It was very interesting to see that after twelve years, and in spite of all her hardships, her back had kept admirably well all this time.

30A. A child, who arrived with thirty-four tuberculous foci—osteitis, periosteitis, adenitis, multiple sinuses, tuberculosis of both feet, right hand, peritonitis, and advanced tuberculosis of left lung. General condition very poor, advanced cachexia, high temperature, and albuminuria.

30B. The same one year later; all tuberculous foci cured.

30C. The same three years later, on his skis.

30D. The same ten years later; cure completely maintained, in spite of the patient having lived throughout the war in a German orphanage, where he underwent considerable privation.

X-ray Evidence.

If these photographic documents bear testimony to the success of heliotherapy, the demonstrations given by the *x* rays which are taken regularly of each patient, and the number of which now attains the total of 13,000, are much more convincing, and show with admirable clearness the process of cure of the most deep-seated lesions in the skeleton, a process which in many cases goes as far as to reconstruct it *ad integram*. I should be very happy to show some of these skiagrams, but unfortunately time does not permit.

May I remind those interested in this question that this most important matter of the radiographic control of the results obtained by heliotherapy will be the subject under discussion in one of the most important chapters of our post-graduate course on heliotherapy at Leysin?

Preventive Heliotherapy.

If sun treatment can cure so many manifestations of tuberculosis, and maintain this cure, it can all the more prevent its outbreak. Experience confirms this. The sun bath, associated with open-air life and rational exercises, is the most active and surest prophylactic means, and for this reason acquires a social value surpassing its therapeutical value.

According to present views it is granted that tuberculous infection occurs, as a general rule, during childhood; reaching the age of puberty nearly all children (95 per cent.) have paid a debt to tuberculosis. Adenopathic and mesenteric ganglia are as outposts, where Koch's bacilli are kept in check. Since the germ of that disease is caught during childhood it is during that same time it must be fought and overcome; Koch's bacilli are incapable of harming so long as the bodily organism is fit to withstand. There is no surer way to attain this state of resistance than by air and sun. That is why, from the first, we have pleaded for its application in nurseries, orphanages, holiday schools, for in the schools, where constant discipline is the rule—discipline so often absent from family education—these principles of hygiene can be applied to children with most profit.

We have put into application this prophylactic means with delicate children and those predisposed to tuberculosis in our "school in the sun" established at Cergnat in 1910. Instead of educating children in rooms more or less without air and light, lessons are given them in open air and sun: this continual open-air life soon completely transforms the children; weak, ailing little beings, a prey to illness, become vigorous, bronzed children, with good musculature, fit to withstand fatigue and inclemency of weather, strongly protected against tuberculous invasion.

[A short cinematographic film was shown illustrating the "school in the sun."]

We know that hygiene is well applied in the schools of this country and the important part it plays in your open-air schools particularly. Our only suggestion would be that a larger place should be given to the sun, so as to place the children's bodies as much as possible in contact with the solar rays, at least during the gymnastic exercises.

This method could, without expense, be widely applied in all the schools. The beautiful parks and public squares, the glory of this country, would also be a splendid field, even if they were only used one or two hours a day for physical exercise in the sun.

Conclusion.

"Where the sun enters the doctor does not go" is an old popular saying, yet it contains quite a programme. Sun is indeed the incomparable remedy that foreseeing Nature has placed within the reach of everyone. It behoves us to make the most of it. The human body, being everlastingly threatened at all ages by many causes of impoverishment, must constantly be armed against them. Let it demand of the air and sun, these fundamental elements of hygiene, the resisting forces it is in need of.

These elements should have a larger place given them, in daily life and in the city. Not only modern towns should be built on such hygienic principles, but also the home. Housing has played an immense part in the genesis of child and adult tuberculosis: the community bears the responsibility of the waste of human lives through insanitary houses. Those principles should be propagated in all classes of society, that all may fully be persuaded that they are indispensable conditions of health and life.

SOME ASPECTS OF THE TREATMENT OF SURGICAL TUBERCULOSIS.

BY

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During the last few years Glasgow, like the other large cities, has been giving more and more attention to the treatment of surgical tubercle. But it has only been since the war ended, when the hospitals which had been given over to the wounded were evacuated and returned to the use for which they were originally intended, that it has been possible to carry out the treatment on anything approaching the necessary scale. For over a year now we have had about 300 beds available for the treatment of surgical cases, and it will be my endeavour to give some indication of our difficulties and our experience of the various methods of treatment which have been recommended in recent years.

Perhaps the most striking results that have been published on the treatment of surgical tubercle are those of Dr. Rollier of Leysin, but his results are obtained where very special climatic conditions exist, and the same may be said, although perhaps to a less extent, of the results obtained by many others. In the course of the next few years this city will have a hospital as favourably situated as is possible within the borders of Scotland, but in the meantime we have to rest content with a hospital which is just beyond the city boundary, where our sunshine reaches us through an atmosphere not by any means free from smoke and dust. It will perhaps be of value therefore if I give some account of our experience of treatment under these less favourable circumstances, for it is likely that some considerable time will elapse before every tuberculous patient in this country can be treated under the most favourable conditions.

I should like to say something of the type of case which we have been admitting to hospital. As I have already explained, it is only recently that suitable accommodation has been provided, and so a large proportion of our cases come to us in a very advanced stage of the disease. They have been resting at home, attending the out-patient department of one of the large infirmaries, and being admitted from time to time to these institutions for scraping of sinuses, excision of joints, or other operative interference. A great many of our cases, therefore, have on admission sinuses which have been discharging for some time, and which have got badly infected. A number of others have abscesses on the point of bursting, and their health is often so undermined that it is almost impossible for us to save them from breaking down.

It was only a year ago that our hospital was opened and that we began systematically to treat cases of surgical tubercle. For a year before this we had a certain number of cases accommodated in a temporary hospital, without facilities for getting the patient into the open air. During this period I tried a group of about forty cases on trypsin without finding any evidence of improvement in any of the cases—that is to say, there was no more improvement in the cases having the injection than in the other cases enjoying the same hygienic conditions; as the treatment was by no means painless I gave it up entirely.

For a year now practically all our patients have been submitted to sun treatment, based on that described by Dr. Rollier in *La Cure du Soleil*. Our patients are gradually accustomed to the sunbath, the exposure being given according to a table, which is exactly the same as that of Rollier, except that fifteen minutes is substituted for his period of five minutes. That is to say, the first exposure is one of fifteen minutes to the feet only, and then on the second day the legs are exposed for fifteen minutes while the feet are exposed for half an hour. On the third day the thighs are exposed for fifteen minutes, the legs for half an hour, and the feet for three-quarters of an hour, and so on until the whole body is exposed. Then we gradually increase the length of the exposure of the whole body until the patients are having a sunbath of six hours a day. There are, of course, many interruptions of the treatment, for we have long spells when we have no sun at all, and in the spring and autumn it is only possible to give short exposures on account of the low temperature. In all cases with sinuses the wound is exposed under a thin piece of gauze.

As regards the results of this insolation, as we have observed it, there is no doubt that the improvement in most of the cases is marked, but we do not obtain the phenomenal results that Dr. Rollier gets. The general health of the child improves, but it takes a long time before one sees much change in the local lesion. We do not see the extrusion of sequestra and the shelling out of diseased glands which he has described. It is easy to understand why this should be, for most of the ultra-violet rays are absorbed or reflected in their passage through our humid smoke and dust laden atmosphere. We have not the increase in the red corpuscles which is observed in people living in high altitudes, and we miss the intensifying effect of the reflecting snow and ice. I find myself in agreement with Lucas, who has been working in California, and Strauss, working in the lowlands of Europe, and believe that in such a climate as ours insolation is of great value as supplementing surgical measures, but that the solar treatment alone is too slow and doubtful to be depended upon. Even Hyde and Lo Grasso, who have reported very favourably of the sun treatment in the lowlands of America, only claim to get movement in joints in which the disease is in an early stage, while Rollier has obtained movement in joints in which there was already fibrous ankylosis.

As regards pigmentation of the skin, most of the patients seem to pigment well, but a number do not. I am not convinced, however, that they make slower recoveries than those who do pigment. It may be that once they have recovered there is less likelihood of a recurrence of the disease in the patient who browns well. This is a point on which I am not yet in a position to speak.

I should like now to refer briefly to our experience of the treatment of patients with cold abscesses. I have already mentioned the fact that many of our patients come to us with abscesses in an advanced stage of development, so that in a number of cases we have found it difficult to prevent sinus formation, and this is especially so during the winter months, when we have a minimum of sun and the weather is cold and damp. For some time we tried the effect of simple aspiration repeated with varying frequency, according to the rate at which the cavity refilled. In some cases this was enough, but in many others aspiration had to be carried on for a very long time before the cavity was obliterated, and in a number the abscess finally broke down. In one case of a large psoas abscess I aspirated the patient thirty-five times, and then the tuberculous infection travelled along one of the needle tracks, and the abscess discharged. In spite of every care infection ultimately took place and I lost the patient.

In a few of the cases of psoas abscess which were not responding to simple aspiration, I tried open operation with a very careful surgical technique—in fact the same technique as is recommended by Lane for the plating of fractures. The abscess cavity was exposed by a long incision, its whole lining carefully cleared away, the walls swabbed with ether, and the wound completely sutured up again. The results were only good in about a third of the cases. The wound began by healing well, but tuberculous infection of the scar took place, and when this broke down it usually resulted in a sinus leading down to the bony focus.

The use of modifying fluids has given us the best results in the treatment of abscesses, and of these fluids the one we have found most useful is the solution of iodoform in ether and oil with creosote and guaiacol, as recommended by Calôt. Occasionally, when the fluid is very thick, we use his preparation of naphthol-camphor, but otherwise his No. 1 fluid is the one we almost invariably use. We have noticed that abscesses which were being treated by simple aspiration over a long time without definite improvement cleared up after a few injections of the iodoform-creosote mixture. One case with a very large double psoas abscess extending into both thighs, who had been aspirated nineteen times, and had frequently as much as 750 c.cm. of fluid drawn off, completely healed after six injections, and this has been our experience with many other cases, although the results were less striking than in this one.

Considering only cases which are in hospital at present, we have 28 which have completed a course of aspiration and injection, and in 20 of these the abscess is completely obliterated, while in 7 others considerable improvement has taken place, fluid in one being reduced from 200 c.cm. to 10 c.cm., and in another from 1,200 c.cm. to 8 c.cm. The cure of these 7 will almost certainly be completed by another course. In one case there has been no response to the treatment. Another 8 cases are undergoing their first course, and are apparently doing well.

The treatment of sinuses is one of the most difficult problems we have to deal with; in the course of a few years I hope we shall seldom see a sinus. At present a large proportion of our cases have sinuses on admission. For the

grossly infected cases one can do little but try to provide free drainage, and depend on sun and air to increase the patient's resistance, but in the afebrile cases we have obtained good results from the use of Calôt's paste. Of 41 cases which are in hospital at present under treatment by this method 21 have healed after one course, 16 are improved, and in 4 the course is still incomplete. Some of the results are quite striking. Two cases of hip disease with sinuses of two years' duration healed after a single course, while another patient with a cavity in the head of the tibia, which had been treated with trypsin and had twice been opened up, scraped, and disinfected with apparently no improvement, healed after six injections of paste. Almost all these sinuses are infected with pyogenic organisms, and those in which the streptococcus is present seem to be the most resistant to treatment.

There is one other method of treatment which we are trying, but about which it is too soon to make any report. During the last two months Dr. Henderson (radiologist to the hospital) has started a series of cases on deep therapy. In tuberculous glands he has already obtained very good results, masses shrivelling to half their dimensions after a single exposure, but we are not yet certain what effect the treatment is going to have on tubercle of bone.

Among the cases that were sent to us for treatment were a number who were clearly syphilitic, while there were many cases in which the diagnosis seemed somewhat doubtful. This led us to carry out Wassermann tests in a series of nearly 300 cases of surgical tuberculosis, and we got a positive result in approximately 5 per cent. We found that we had to be extremely careful in the antisyphilitic treatment of patients who had a positive reaction and yet had a definitely tuberculous lesion. The injection of arsenical preparations had to be given in very small and carefully graduated doses; in fact in some cases it was impossible to give any injection at all, as the tuberculous lesion was liable to be rendered more active, old sinuses to break down, and new abscess formation to take place.

I have not referred at all to the orthopaedic treatment, which must be carried on at the same time as the general. Indeed, a very important part of the treatment of abscesses and sinuses is the immobilization that is obtained by suitable extension or plaster apparatus. I have been much impressed with the fact that even such improved hygienic conditions as we are able to offer, and the limited sunshine we have at our disposal, so increases the patient's resistance that, without lighting up fresh tuberculous mischief, we are able to correct by careful manipulation deformities which we had failed to correct by extension.

In conclusion, I should say that, while the treatment of surgical tubercle must anywhere be essentially conservative, in hospitals in which one has not the most favourable climatic conditions, operative interference combined with general treatment will give the most speedy, although perhaps slightly less perfect, results.

I have to express my indebtedness to Dr. Watson, superintendent of Robroyston Hospital, for his helpful advice, and for facilitating my work in every way, and to the house-surgeons for the excellent way in which they have carried out the treatment and kept the records.

SECTION OF PHYSIOLOGY.

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DISCUSSION ON BASAL METABOLISM.

OPENING PAPER

BY

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BEFORE that very interesting and pertinent question, What is the significance of variations in the basal metabolism? can be answered much work requires yet to be done. The observation, which has been well established from data collected from all parts of the world, that there is a striking uniformity in the energy requirements of classes of people—for instance, farmers doing the same amounts of muscle work—suggests the possibility that a similar uniformity will be found in the basal metabolism—that is, the metabolism during complete muscular rest in the post-absorptive condition. The work of Du Bois and many others all points in the direction of uniformity. If this uniformity be well established then the answer to the question will certainly be simplified, but no light will have been thrown on the nature of basal metabolism. In my contribution to the question I shall try to deal with some of the physiological phenomena associated with the consumption of oxygen and leave to others the clinical aspects of the question.

I think it may now be stated without fear of contradiction that the trend of modern work is to break down the old-time barriers which were believed to exist between the metabolism of the different proximate principles. The "active" substance of the cells is the complex material protoplasm, a substance which incorporates within its structure protein, carbohydrate, and "lipoid" material. Further, the work of Dakin and others has shown that the intermediate products of the breakdown of carbohydrates can be utilized in the synthesis of protein. It is now also beyond question that proteins in their turn can give rise to carbohydrates, and probably also to fat, as carbohydrates can undoubtedly form fat. Again, evidence—at present more indirect it is true—is slowly accumulating which shows that fats in their turn can give rise to carbohydrate, and therefore may take part eventually in protein synthesis.

It would seem, then, that in the metabolism of the animal, with this apparently free interchangeability between the various proximate principles, we have a state of immense resiliency, and that very wide fluctuations in the nature of the diet may be possible without casting an undue strain on the organism. Such is the deduction, and all the prolonged experiments on starvation would go to prove that such is the case when the organism has its own choice of material to call upon. But if, instead of throwing the tissues, so to speak, on their own resources—drawing for their energy and their wear and tear quota on the stores of the body—drastic changes are made in the type of food material supplied, the resulting changes are quite different. The organism is flooded with potential food material of one type, and in the attempt to utilize or get rid of it profound changes in metabolism result.

When we confine our attention exclusively to the variations which occur in the composition of the urine we are definitely limiting our field of vision, we are confining our attention to a single facet—namely, protein. The urine is practically confined (disregarding for the time the inorganic constituents) to the disposal under normal conditions of the waste products of protein metabolism, and it is only when the tissues are flooded with abnormal products, as in acidosis, that other material is excreted. It is undoubtedly true that profound changes in the composition of the urine can take place. Whatever else happens the course of the metabolism of protein can apparently be drastically interfered with.

The other two channels of excretion through which we gain evidence of changes in metabolism are heat loss and the respiratory exchange—that is, intake of oxygen and output of carbon dioxide. In many respects the information which is to be obtained from the study of the respiratory exchange throws a much more brilliant light on the intracellular changes than does either the study of the heat output or the end-products of protein metabolism in the urine. In the latter we are dealing only with the waste products of intracellular activity, whereas in the former—the respiratory exchange—we have to consider not only the waste carbon dioxide but the intake of oxygen which is absolutely essential for the running of the organism. Unfortunately the difficulty we encounter here is the manner of the distribution of the absorbed oxygen between the combustion of the various proximate principles—in other words, the interpretation of the respiratory quotient. The relation between the intake of oxygen and the output of carbon dioxide is a mere ratio. Unfortunately there seems to be a tendency amongst some workers to lose sight of the fact that the respiratory quotient is a quotient and nothing more. The respiratory quotient (R.Q.) does not represent a single change; it is a quotient of probably many components, the result of innumerable activities, the summation of many and varied metabolisms, although of course it is equally true that at times and under special conditions it may be so drastically altered that it may be said to represent a metabolism predominantly carbohydrate or predominantly fat. But even in these cases, with the R.Q. at the so-called 100 per cent. carbohydrate or fat level, it is not to be inferred that all other metabolic activities are in abeyance; we may not assume that, although ostensibly there is 100 per cent. combustion of fat, no other combustion is taking place. It is a self-evident fact that all tissues are living, that the active metabolic

"furnaces" are the muscle cells, that so long as a tissue is living it requires an intake of oxygen; carbohydrate, far less fat, cannot be expected to burn, so to speak, on a cold hearth.

The problem, too, is not one of the possible combustion of the elaborate molecules of carbohydrate or of fat but of intermediate products of these, probably of relatively simple and, in the end, very similar products. The intake of oxygen for the so-called combustion of fat is not only required for the actual combustion process but for participation in a series of preparatory oxidative changes, in which the oxygen-poor fat is converted into a series of oxygen-richer molecules, which can, when the demand arises, be readily utilized by the active metabolic cells.

The laws of Berthelot and of Hess, although they undoubtedly express general truths which may or may not be universally applicable, just like the ordinary everyday use of symbols in the expression of chemical equations, tend, when used without due thought, to confuse the issue. Thus the chemical equations commonly employed to express the complete combustion of carbohydrates and fat—

$$C_6H_{12}O_6 + 6O_2 = 6CO_2 + 6H_2O. \quad R.Q. = 1.00;$$
$$\text{and} \quad C_{55}H_{104}O_6 + 78O_2 = 55CO_2 + 52H_2O. \quad R.Q. = 0.707$$

—probably do express, more or less correctly, the amounts of oxygen required for the complete combustion of these substances and the amounts of carbon dioxide which result from this combustion; but we neither obtain any clue to the actual chain of events which take place nor to the possible variables in these changes. It is well recognized that we can get respiratory quotients much below 0.7 and above unity. Thus in the possible conversion of carbohydrate to fat there may be an output of "atypical" carbon dioxide expressed by a chemical equation, on paper at least, as follows:

$$4C_6H_{12}O_6 = C_{16}H_{32}O_2 + HCOOH + 6CO_2 + 6H_2O,$$
$$\text{or} \quad 13C_6H_{12}O_6 = C_{55}H_{104}O_6 + 23CO_2 + 24H_2O,$$

which has nothing to do with any combustion, but with a mere intramolecular arrangement. Or, on the other hand, we may consider the excessive intake of oxygen requisite for the conversion of fat to carbohydrate, presuming that the deductions of Pembrey, Krogh, and others are sound, with a correspondingly low output of carbon dioxide—

$$2C_{55}H_{104}O_6 + 60O_2 = 16C_6H_{12}O_6 + 14CO_2 + 8H_2O. \quad R.Q. = 0.23.$$

When the possible fates of protein are considered the chemical formulae for their expression are wellnigh endless, and, as these various mutations are for the most part even more hypothetical and obscure than the foregoing, they may be neglected.

Too often the assumption, implicit perhaps, is made when metabolism is under consideration that we are dealing only with a series of katabolic phenomena, whereas metabolism is a blend of anabolic and katabolic processes, with the balance weighted, in the healthy, well-fed organism, slightly in favour of anabolism. The stimulation of the metabolism by various agencies is constantly referred to, but what does the statement really mean or imply? What is basal or standard metabolism, and what relation does it bear to the average daily metabolism?

The term "basal metabolism" has been objected to by Krogh, who has suggested the introduction of the term "standard metabolism" on the ground that "the basal metabolism of an organ is not a constant quantity but can be modified experimentally by varying the external conditions (for instance, the temperature) and may probably vary also from internal causes." Krogh by his term "standard" means to imply simply "that all variations in metabolism brought about by functional activity have to be referred to this quantity as a standard." He finds, speaking generally, that true basal metabolism is about 75 per cent. of the standard—that is, it represents the standard metabolism as ordinarily determined less the aggregate influence of functional activities like cardiac or renal activity, respiratory movements, etc., in the resting body.

True basal activity, then, would represent the sum total of the various changes which are taking place in the cells, simply for the purposes of maintenance, the ordinary wear and tear, and the repair of the same. When food is given or functional activity increases, does a mere stimulation of the basal metabolism, with a corresponding alteration of its character depending on the nature of the stimulus, take place, or is there a new type of metabolism superimposed on the basal? To take a rough simile—are we dealing with an arrangement analogous to a gas fire supplied with a gas of constant composition and at a uniform pressure (which may from time to time but very gradually vary)? Let this lighted gas fire represent the basal metabolism: Is the metabolism of functional activity to be represented by, let us say, a spray of a combustible fluid sprayed directly on to the flame or by the combustible material being slowly introduced into the main gas supply? In the one case there would be a sudden burst of flame and heat without any material alteration of the original fire, a metabolism superimposed on the basal; or, in the second, we would have the actual character of the basal flame altered materially, according to the nature of the combustible substance introduced, and for a relatively long period; in other words, the basal and added metabolisms would be integral parts of the total metabolism. To my mind this is probably the most fundamental problem in the whole field of metabolism.

Benedict has made the general statement, with which I whole-heartedly agree, that basal metabolism is a function of the active protoplasmic mass of the organism. Few workers will, I think, be found to dissent from this sweeping statement. But what does it actually mean? what activities are comprised within this doctrine? It is undoubtedly true that the basal metabolism remains very constant over very long periods. Thus A. Loewy's figures, which extend over a period of fifteen years, are extraordinarily constant for oxygen intake in cubic centimetres—1888, 236; 1895, 228; 1901, 231; 1902, 238; 1903, 228. Both Magnus Levy and Johansson found a similar regularity over shorter periods, as did also Benedict and Cathcart. On the other hand, it has been shown just as definitely that by special treatment, such as a compulsory lowering of the food intake or of drastic alterations in the composition of the food, the basal metabolism may be markedly altered.

We may take it that the true basal metabolism is approximately a constant, and that in all probability it corresponds to the minimum chemical activity which is commensurate with the maintenance of life. The heat developed may be regarded as a necessary product, but, as Lambling puts it, not the physiological aim. For, be it noted, the external temperature at which the minimum is reached is not that of the organism itself, but is the temperature at which there is, to all intents and purposes, a balance between the heat loss and the minimal production of heat. When such a balance is obtained, as in the experiments carried out by Lefèvre, it was found that the standard metabolism was reduced by about one-third.

We have now to consider a question which is of immense importance to the clinician in the interpretation of the results obtained by the determination of the basal metabolism. Is there any variation of any kind in the so-called basal metabolism, and what is the effect of the different proximate principles on the metabolism?

Although the observations of Loewy already cited show that the basal metabolism remains approximately constant over many years, these observations were not intended to be more than approximations. It is obvious that in the consideration of variation one should inquire whether there is, in the first place, any diurnal variation in the metabolism in any way comparable to the diurnal variations which occur in temperature, pulse rate, and, as Leathes and others have shown, in protein metabolism. Johansson, Landergren, Sonden, and Tigerstedt, many years ago showed well that, irrespective of whether the subject is fasting or fed, a definite reduction in the metabolic rate takes place during sleep. One of the most complete proofs of this observation are the long series of experiments which Benedict carried out daily on a fasting man over a period of thirty-one days. He found that the average increase of waking metabolism over sleep was just over 13 per cent. (varying from 27 to 4.5 per cent.). He further noted that the subject had a much greater intake of oxygen in the late afternoon than in the morning.

Figure 1 shows a most curious and interesting relation between the diurnal variation in the basal metabolism, the heat output as determined by rectal thermometer, and the protein metabolism as determined by the output of uric acid. It is an interesting speculation as to whether the variation in the basal metabolism is determined by the diurnal variation in the metabolism of the cell constituents, or whether the variation in the output of nitrogen is conditioned by variation of the causal factors underlying and determining the basal metabolism. or, finally, whether they are simply parallel

changes with little or no causal relationship. Although it might be argued that the fall in the oxygen consumption during sleep is due to the reduction in the gross muscle activity, the fall in the output of nitrogen is almost certainly not due to this reduction in activity, as it has repeatedly been shown that the output of total nitrogen is rarely increased, provided the supply of appropriate food is adequate, even after the most strenuous work. Leathes also showed that the output of total nitrogen was highest at night.

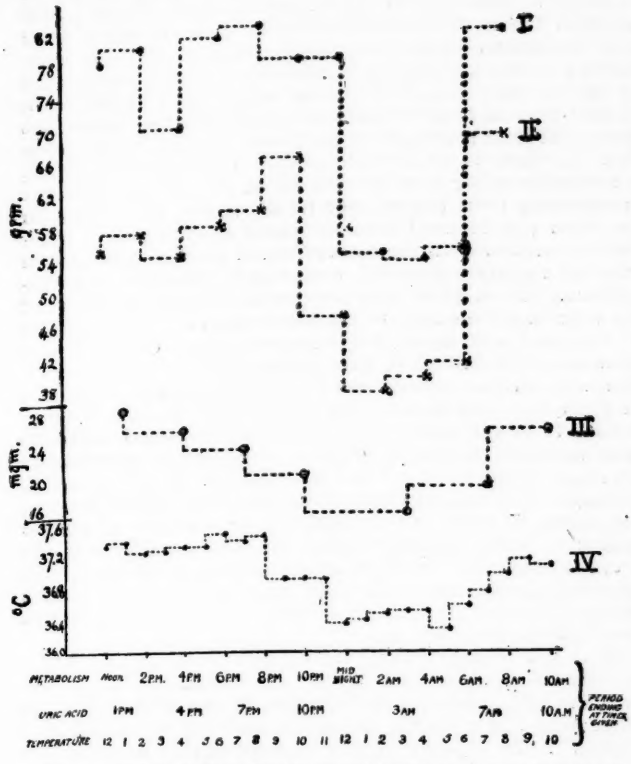

FIG. 1.—I and II, Carbon dioxide output (I with food, II without food) in grams per two hours (Johansson, Landergren, Sonden, and Tigerstedt). III, Output of uric acid in milligrams per hour (Leathes). IV, Body temperature in degrees centigrade (Benedict and Snell).

Curiously enough, Benedict found a figure almost identical with the sleep-waking difference figure—namely, *circa* 13 per cent.—for the percentage variation of thirty-five "normal" men who had been repeatedly examined (in all over two thousand times) over periods varying from $4\frac{5}{12}$ years to only 5 days—namely, 13.9 per cent. The average variation in the three longest periods was $4\frac{5}{12}$ years, 14.9 per cent.; $2\frac{5}{12}$ years, 25.9 per cent.; $2\frac{3}{12}$ years, 12.1 per cent.; and in the three shortest periods—5 days, 15.3 per cent.; 6 days, 8.1 per cent.; and 12 days, 4.9 per cent. The subject whom Benedict and I studied practically daily for four consecutive months had a variation of over 20 per cent.

Further, the series of observations of Palmer, Means, and Gamble, which incidentally point to the influence of the time of the year or a seasonal variation, are of considerable interest. These workers found that with a subject whose body weight remained practically constant summer and winter (variation of 0.2 kg.), the heat output per kilo was 21.4 calories in the winter and 19.2 calories in the summer. The determinations were made under practically identical temperature conditions.

It is, of course, well known, and disputed so far as I am aware by no one, that the average woman has a lower metabolic rate than the average man, and that children have a higher metabo ism than adults. Moreover, Benedict has shown that athletes have a higher metabolism than non-athletes of comparable age and weight.

The other question as to whether variations result in the basal metabolism from variations in the nature of the food consumed is also of primary importance. It is to-day a commonplace to say that the information to be gained from the examination of a chance sample of urine is negligible, and it is also true to say that the information to be gained, even from the examination of a sample taken from a twenty-four hours' specimen, is of little value unless the nature of the diet taken by the subject is known. In my opinion this is equally applicable to the determination of the basal metabolism. It is perfectly true that basal metabolism is always determined in the post-absorptive state—that is, twelve to fifteen hours after food; but it is equally true that the condition of the body, the state of nutrition of the cells, is solely dependent on the food ingested, and it is very questionable if the cell contents will have reached a state of constant composition within the time given, irrespective of the previous nature of the diet.

It has been shown that the body has no great capacity for the storage of protein, and it has been stated by Rubner that protein deposited in the form of new tissue exerts no specific dynamic action. Still, the fact remains that protein ingested, as superimposition experiments have clearly shown, is not disposed of at once, that the output following the ingestion may be high for several days after, the duration of the excretion being dependent on the nature of the protein consumed. It is also true, as the following figures from Cathcart and Orr's paper clearly show, taking the Du Bois figure of 39.7 calories per square metre as an average figure, that the basal metabolism, strictly determined, is higher on the day following the ingestion of protein than on the days following the ingestion of either carbohydrate or fat.

	O_2 Intake.	Cals. per sq. metre
Mean of all experiments on subject M. ...	242 ...	39.8
Ordinary mixed diet	252 ...	40.9
Carbohydrate diet	210 ...	35.6
Fat diet	212 ...	35.1
Protein diet	274 ...	44.8

Again, it is, of course, a well-documented "fact" that protein acts on ingestion as a direct stimulant of metabolism, and that, as the brilliant work of Lusk has so clearly demonstrated, most if not all of this effect can be referred to certain amino-acids. As regards the effect of the previous ingestion of carbohydrate or fat, both of which can be stored in the organism, in my opinion the answer is equally plain.

Benedict and Cathcart record in their experiments with M. A. M., whose average oxygen intake was 240 c.cm. a minute, that on the high, but not absolutely pure, carbohydrate diet the average intake was 234 c.cm., and on the carbohydrate-poor diet it was 250 c.cm. The experiments of Krogh and Lindhard are also of interest in this connexion; an analysis of all their data shows fairly conclusively that quite a marked difference exists between the oxygen intake on a carbohydrate-rich and a carbohydrate-poor diet. In three groups out of five the oxygen intake per minute on the carbohydrate-rich diet is very definitely below that on the carbohydrate-poor diet, and on the other two the intake on both diets was practically identical. The average of all the groups (including figures where the subject is stated to be on a fat diet, but where the R.Q. definitely shows that the glycogen reserve is being freely drawn upon, a reputed fat diet day with an R.Q. above unity) is 245 c.cm. oxygen a minute on the carbohydrate-rich and 257 c.cm. on the carbohydrate-poor days. Krogh and Lindhard, as the result of their work, conclude that the standard metabolism is not independent of the preceding diet. They found that when the diets are protein-poor the metabolism is lowest at intermediate quotients, and that it increases about 5 per cent. when the quotient falls to about 0.71 and about 3 per cent. when it rises to about unity. They also hold that their work shows that in this non-protein metabolism, both during rest and during work, the proportion of fat to carbohydrate katabolized is a function of the available supplies of these substances.

It is obvious, then, that the state of nutrition as regards the stores of food material in the tissues at the time of examination, even if the subject is in the post-absorptive state, plays a part in determining the level of the standard or basal metabolism, and that the nature of the previous diet is a factor which cannot be neglected in the final assessment. Is the converse true—namely, that when all the readily available stores are depleted and the organism is running at a low level the basal metabolism is depressed? This, again, is a problem of profound interest to the clinician, as so many of his patients are in a poor state, undernourished, or poorly developed physically. Benedict's figures, to quote a single series, show definitely that as the result of undernutrition the basal metabolism is undoubtedly below the so-called normal level—namely, the Du Bois standard. It follows that mere attention to the level of the metabolism, neglecting the physical state of the subject, may lead to very erroneous conclusions.

Still another factor which comes into operation and cannot be disregarded is the nature of the work done by the subject prior to the examination—not merely immediately prior, but on the previous day. Has he been active or at rest? What has been the duration of the rest period, and what has been the nature of the "rest" before the actual examination is made? What kind of night has the patient had? Have hypnotics been used? There is no doubt in my opinion about the prolonged after-effect of work. It is true that, if the pulse be taken as the guide of quiescence, very often it rapidly, or fairly rapidly, returns to its normal figure; but this is no true index of the effect of muscle work on the various metabolic activities. Just as it has been shown in certain experiments on protein metabolism that a slight rise in the output of nitrogen may take place several days after the spell of work has been carried out, so there may be a resultant increased, though gradually diminishing, consumption of oxygen extending over many hours after the cessation of work, when all the more generally recognized post-work symptoms have completely disappeared.

The real difficulty which confronts the modern worker in his interpretation of the value to be attached to basal metabolism data is that we have no very clear indication as to the causal relationships between cell activity, oxygen consumption, and heat output. In conclusion, then, I wish to put before you certain considerations, largely it is true of a hypothetical nature but based on experimental data. In that muscle forms the most important and most uniform bulk of active metabolic tissue it will be perhaps useful to see if any possible explanation can be given, using this tissue as type material.

Grützner in 1887 had adumbrated the idea that there might be two different parts of muscle involved in contraction, an idea which was put forward much more definitely by Bottazzi in 1901 as the results of his experimental work with veratrine. He suggested that there were two active substances in muscle, (*a*) the doubly refracting material, and (*b*) the sarcoplasm; the first of these, being very irritable, is responsible for active movements, whereas the second, much less irritable, accounts for tonus. A few years later Mosso, on the ground of Perroncito's observation that apparently muscle was innervated by two types of nerve fibre, suggested that muscle might respond differently, depending on the source of the stimulus. This work on the possible double innervation of muscle has been investigated by many different workers, but so far without leading to any general agreement. One of these workers, de Boer, came to the conclusion that tonus is associated with the sarcoplasm, is carried on metabolically at the cost of a slow utilization of protein, and is under the control of the sympathetic system; whereas the rapid movement is the result of stimuli passing down the motor nerve, leading to rapid combustion of carbohydrate in the anisotropic substance.

Other work which throws some light on the complicated problem is that in which curare has been utilized. Many years ago Zuntz and Pflüger both stated that the administration of curare brought about a marked diminution in metabolism. This conclusion was controverted by Frank and Gebhard, and later by Frank and Voit, who found that provided the dose of curare was not too great there was but little diminution in metabolism—a statement confirmed by the excellent work of Tangl. Mansfeld and Lukács, in the light of the possible double innervation and the fact that curare in moderate doses only affects the motor nerve endings, re-examined this question. They determined the gaseous metabolism of an animal given a dose of curare which just sufficed to abolish the capacity of muscle to respond to indirect stimulation before and after complete section of all nerve supply to the muscles of the hind limbs. They found that although this procedure brought about a denervation of only about one-third of the total musculature of the animal a marked reduction both in the intake of oxygen and the output of carbon dioxide took place; no such reduction occurred when the sympathetic supply alone was destroyed. In a later paper Mansfeld found that with massive doses of curare the sympathetic nerves can be put out of action. Incidentally he suggested that the so-called mechanical tonus is identical with chemical tonus. Still later Ernst, working under the direction of Mansfeld, published the results of an investigation into the nature of the source of energy for tonus and rapid contraction. He found that the glycogen content of muscle rapidly diminished as the result of stimulating the sciatic nerve, but that if a dose of curare were given (just sufficient to throw the motor nerve out of action) stimulation no longer caused the disappearance of glycogen; indeed, in some cases, the stimulated side actually contained more glycogen than the "normal" side. The inference to be drawn is that the chemical expression of rapid contraction and of tonus is different.

It would seem that there is a good deal of evidence, both direct and indirect, in support of the view that the activity of muscle is not a single simple phenomenon, but that we have to deal with at least two types of activity, one of which seems to be associated with the sarcoplasm, and to which the term "tonus"—plastic either in the sense of Sherrington or of Langelaan—is applied, and the other, which is associated with the sarcostyles, the activity of which is evidenced by the ordinary rapid contractile response. I do not for a minute pretend to decide between the conflicting views, but there is at least presumptive evidence in favour of the view that the first is under the control of the sympathetic, whereas the other is under control of the ordinary efferent motor nerve from the anterior horn. Again, the suggestion has been put forward that the tonic type is mainly associated somehow with the metabolism of protein, whereas the other is mainly associated with the metabolism of carbohydrate. The work of Pekelharing is certainly in favour of the view that the output of creatinine is in some way related to "tonus," and again the experiments of Cathcart and Leathes on the effect of involuntary work, in the form of shivering, on the distribution of nitrogen in the urine, tend to show that the metabolism of uric acid is of a peculiar order. It is very interesting that both of these substances, creatinine and uric acid, were found by Folin, according to his well-known work on the composition of the urine, to belong to the limited class of substances to which he gave the name of tissue or endogenous or constant metabolic products. Quite rightly, I think, he held the view that the demand or need for protein by the organism is small, and he clearly demonstrated that the more the total nitrogenous metabolism is reduced the more prominent do these constant metabolic products become.

Is it too presumptuous to suggest that these various endogenous products, not merely creatinine, actually represent the effects of the metabolism of the sarcoplasm—in other words, they are the metabolic products associated with "tonus"? In support of this hypothesis may be submitted the very interesting observations of Leathes, which were later confirmed by Wolf and Osterberg, on the diurnal variation in the output of creatinine and uric acid already referred to. It was found that the uric acid and also creatinine output was lowest during the hours of sleep—that is, of the most complete muscle rest—at the period when, to use Sherrington's interpretation of tonus, the postural contraction would be least in evidence. It is, of course, a fact of common knowledge that "tonus" is at its lowest during deep sleep (Lombard's work on the knee-jerk, for instance), just as it is reduced in positions of complete rest during the waking period. I should like to go even farther and suggest that the true basal metabolism is a phenomenon separable from metabolism in general, and that it is based for the most part on the sarcoplasm or "tonus" metabolism. It is generally admitted to be the lowest limit of metabolism compatible with normal tissue activity, and that it is certainly a function of the mass of the active metabolic tissue, of which muscle is the predominant material.

It is found that, just as is the case with uric acid and creatinine, there is a marked diminution in the gaseous metabolism during the hours of reduced tonicity—that is, during sleep (see Fig. 1). This is well established by the experiments of Johansson, Landergren, Sonden, and Tigerstedt, and of Benedict. If it be demanded that the heat output shall fall into line with these observations, then the continuous records of Benedict and Snell show this to be the case (see Fig. 1). The general result, which could readily be added to, is well summed up in Fig. 1, which I have constructed from the above observations. It is realized full well that such a hypothesis is in no way an explanation of one of the most interesting problems of physiology, but it is put forward in the hope that it is a contribution towards a narrowing of the issue. If it serves to emphasize the danger of drawing too far-reaching conclusions from basal metabolism data, from all types of cases and all kinds of patients varying physically and psychically, it will have served a useful purpose.

DISCUSSION.

Dr. J. B. ORR (Aberdeen) referred to some of the difficulties in the accurate determination of true basal metabolism, particularly to the influence of the psychic condition of the subject and the state of the endocrine glands. The theoretical lowest level of metabolism was unattainable under experimental conditions. In endeavouring to measure very low levels of metabolism we were really dealing with a pathological condition, because intake of nourishment and reactions to other stimuli, either chemical or nervous, were normal functions of the cell. He disliked the conception of normal metabolism as true basal metabolism with an addition superimposed. True basal metabolism was an abstraction. What we really studied was different levels of metabolism with varying degrees of intensity of many functional activities. In studying the influence of any factor on the rate of metabolism it was of course necessary to have a fixed base line, but that was merely an arbitrary condition where, as far as possible, the different factors that influenced metabolism were controlled, preferably at a low level, and specified.

Professor J. C. MEAKINS (Edinburgh) said that his own observations had been chiefly made in pathological conditions. The value of determinations of basal metabolism depended on the care with which they were made. If proper precautions were not exercised a valuable method for the study of disease might fall into disrepute. He described the conditions of observations in his wards and emphasized the importance of mental apprehension as a disturbing factor. It was sometimes necessary to accustom the patient to the procedure by dummy experiments.

[Professor Meakins showed slides demonstrating abnormal levels of basal metabolism in thyroid disease and the changes due to treatment and operative interference. The effect of injections of adrenaline on basal metabolism, respiratory quotient, and blood-sugar content was also pointed out.]

Dr. G. B. FLEMING (Glasgow) discussed the influence of growth on the basal metabolism. He explained that the energy expended in growth must be included in determinations of basal metabolism in children. This probably accounted, to a large extent, for the high basal metabolism found in growing children. He showed that as the rate of growth decreases the basal metabolic rate also decreases.

Dr. LEONARD HILL (London) said that basal metabolism as determined by the respiratory exchange was liable to be raised by the inevitable obstruction to breathing due to the apparatus used. It could also be affected in the same way by voluntary hyperpnoea. Figures for the basal metabolism of subjects under many normal conditions were given. He then went on to discuss the influence on basal metabolism of rate of heat loss, quoting experiments where this factor was measured by the kata-thermometer. In the tropics basal metabolism might be one-third lower than in temperate climates on account of this. The rise in basal metabolism due to more rapid heat loss constituted an important part of the benefit resulting from open-air treatment. The effect was believed to be attributable to the open air and not to sunlight.

Dr. C. M. WILSON (London) referred to the question of the depth of pulmonary ventilation as affecting basal metabolism and of standards for patients in a state of real emaciation. In several patients suffering from menorrhagia with a high initial basal metabolic rate the exposure of the thyroid to x rays had produced a markedly beneficial effect.

Professor CATHCART, in reply to the points raised, said that the technique of determinations of basal metabolism required experience.* The rate of pulmonary ventilation might exercise an enormous effect. Sleep lowered the figures obtained as much as 15 per cent. At present we did not know what basal metabolism really was, but it was certainly the result of the interaction of a large number of factors. Personally he considered the question of muscle tonus as one of the most important of these.

* The portable basal metabolism apparatus used is the latest type of Dr. F. G. Benedict as modified by Roth and Collins. It can be obtained in this country from Warren Collins, Boston, through Messrs. Thomson, Skinner, and Hamilton, Glasgow.

THE DIAZO-REACTION, WITH SPECIAL REFERENCE TO URINE IN MEASLES.

(PRELIMINARY NOTE.)

BY

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THE cause of the diazo-reaction in urine has been attributed to a variety of substances. Clemens (quoted by von Noorden[1]) claims to have isolated the sodium salt of the substance responsible for the reaction. This substance contained 4.5 to 5.4 per cent. nitrogen. Others assume it to be due to chromoxy-proteic acids or to ill-defined members of this group of substances (Neubauer-Huppert[2]). Weiss[3] is emphatic that urochromogen is the substance responsible for a positive diazo-test. Engeland[4] isolated from 40 litres of normal human urine a small amount of histidine. He thus suggests that iminazole derivatives primarily cause the diazo-reaction in urines. Hermanns and Sachs[5] consider that in urines from tuberculous patients an oxidation product of tyrosine is responsible for the diazo-reaction. In the urine of a patient with acute liver atrophy these observers found oxyindolacetic acid.

The methods of carrying out the diazo-reaction in urine have been very varied. Suffice it to note here that, to avoid deaminization of colour giving amino-acids by the free nitrous acid of the diazo-reagent, the latter should be made alkaline before the addition of the urine to be tested. For reasons stated by Koessler and Hanke[6] sodium carbonate is for this purpose preferable to sodium hydroxide or to ammonium hydroxide.

So far as the author is aware, the diazo-reaction in urine has not previously been studied from a quantitative aspect. To find out the relative importance of the substances in measles urine giving a colour with the diazo-reagent a quantitative procedure has been attempted.

Five hundred c.cm. of a mixed sample of measles urine were quantitatively fractionated by lead acetate, lead acetate and baryta, and by mercuric acetate. A portion of the last fraction was further precipitated with silver nitrate, followed by silver nitrate and baryta. The colour value developed by the Koessler and Hanke[6] diazo-reagent was measured in the various fractions. The colour standard employed by these workers for histidine was here used as a standard. An orange light filter placed over the eyepiece of the Duboscq colorimeter was consistently used throughout the measurements in order to obtain a match with the yellow colour developed in certain fractions. The total colour value of the original 500 c.cm. of urine was found to be 86,500 mm.

The lead acetate fraction had a total colour value of 770 mm., the lead acetate baryta fraction a total colour value of 4,400 mm., the mercuric acetate fraction a total colour value of 87,500 mm., and the final filtrate a total colour value of 10,000 mm. The sum-total colour values of the various fractions is greater than the original for the reason that there are certain substances in the original urine which inhibit colour production. The portion of the mercury fraction taken for the silver baryta separation had an original total colour value of 12,600 mm. In the silver baryta fraction there were recovered 12,500 mm., or almost 100 per cent. The silver fraction and the final filtrate gave no colour.

From 1 litre of urine the silver baryta fraction was obtained as above. Creatinine was separated by precipitation with picric acid, the filtrate retreated with silver baryta, and the latter fraction finally precipitated with mercuric chloride. From this well-washed precipitate there was obtained about 0.1 gram of pure histidine monohydrochloride. (The amount actually present per litre was estimated at 0.31 gram.) It had the following properties: melting-point, 253° to 254°; melting-point, with decomposition, of the picrolonate, 231° to 233°. The microscopic appearance of both these salts agreed with authentic descriptions and with the same salts of histidine obtained from a different origin. There was found 16.9 per cent. (calculated 17 per cent.) Cl. The molecular colour value was 114.5 million mm. (Hunter).[7] It was positive to Knoop's test.

The substance mainly responsible for the colour not accounted for by histidine was not isolated, but partly purified and relatively concentrated solutions gave reactions indicating a phenolic constitution.

Conclusions.

1. In the urine from patients suffering from measles, histidine is responsible for about 85 per cent. of the colour developed by the diazo-reagent.

2. In measles the output of histidine in the urine is increased.

The writer is indebted to Dr. Elliot, of Ruchill Hospital, Glasgow, for the material, and to Professor E. P. Cathcart for much help throughout the work.

REFERENCES.

[1] Von Noorden, *Metabolism and Practical Medicine*, vol. ii, p. 162. [2] Neubauer-Huppert, *Analyse des Harns*, p. 1303. [3] Weiss, *Biochem. Zeit.*, vol. cxii (1920), p. 61. [4] Engeland, *Zeit. für physiol. Chem.*, vol. lvii (1908), p. 49. [5] Hermanns and Sachs, Ibid., vol. cxiv (1921), pp. 79 and 88. [6] Koessler and Hanke, *Journ. Biol Chem.*, vol. xxxix (1919), p. 497. [7] Hunter, *Biochem. Journ.*, vol. xv (1921), p. 689.

SECTION OF DERMATOLOGY.

H. LESLIE ROBERTS, M.D., C.M., President.

DISCUSSION ON THE ETIOLOGY OF SEBORRHOEA AND SEBORRHOEIC DERMATITIS.

OPENING PAPERS.

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THE subject of seborrhoea was discussed in this Section of the British Medical Association in 1901 at Cheltenham, when Colcott Fox introduced the discussion and Sabouraud and others took part in it. For some years prior to that much had been published, especially on the bacteriological side of the question, notably by Unna and Sabouraud. Some of that work has been confirmed since, and is generally accepted now, but many of the points raised then are still undecided, and unfortunately the whole subject is still in rather a confused condition.

So many different conditions are included under the terms "seborrhoea" and "seborrhoeic dermatitis" that it is necessary, in discussing their etiology, to divide them up into their clinical varieties. They fall into two groups—namely, (1) the true oily seborrhoea, using the word in its true meaning, and (2) the so-called seborrhoea capitis and seborrhoea corporis or seborrhoeic dermatitis. Both these groups have certain things in common, and it is impossible in discussing etiology generally to keep each clinical variety separate. As to the clinical lesions, I think most of us will be in agreement, but it is quite another matter when it comes to their interpretation.

Strictly speaking the term "seborrhoea" should only be applied to the oily condition of the skin due to increased secretion of sebum. Seborrhagia, steatorrhoea, flux sebacea (Rayer), and seborrhoea oleosa (Hebra) are the older names for the condition. It affects chiefly the scalp, face, central area of the body, back and front, and the flexures, and is generally accepted now as due to increased secretion of the sebaceous glands. A close inspection of the skin will reveal the large and patulous orifices of the sebaceous glands and the oily condition of the skin and scalp hairs. But as it is often associated with increased sweating as well, it is difficult to dissociate the two conditions. Unna prefers the term "hyperidrosis oleosa," as he believes that the greater part of the oil is derived from the sweat glands. No great support has been given to this theory as it does not agree with the clinical facts. Probably there is right on both sides, the increased sebaceous gland activity being accompanied by some hyperidrosis.

This oily condition of the skin is the basic factor in all forms of seborrhoeic eruption. Is it an increase of a physiological process or is it due to an inflammatory infection of the oil-producing glands? The work of Sabouraud, Unna, and others has established the fact that a bacillus is always present in the sebaceous material in such cases, but, although Sabouraud claimed that the organism caused the condition, the tendency to-day seems to be to disagree with that view. All the evidence points to the condition being an amicrobic functional disturbance in the first instance. Before birth the sebaceous glands are very active, as is shown by the vernix caseosa, and at birth the infant frequently shows fatty plugs in the sebaceous gland orifices of the face, especially the nose. To this cause also the greasy adherent scales on the scalp of young infants—crusta lactea—are probably due. No one would suggest that these conditions at birth are of microbic origin. From birth on till puberty oily seborrhoea is not much in evidence, but with the development of the sexual glands the sebaceous glands become fully developed and the hair grows on the face, etc. It is difficult to estimate what is the normal oiliness of the skin, and there is no standard by which we can go. In some individuals the skin is probably naturally very much oilier than in others. It may be an hereditary characteristic. It is well known that the dark races have much oilier skins than the white. You may get ichthyotics with dry skin and deficient skin-gland secretions; why not other skins with increased glandular development? On the other hand, there may be other acquired factors which stimulate the skin glands.

Foods.—That diet is important no one will deny. Certain foods in excess, such as too much fat, starch, or sugar, have been blamed for making the skin oily. If a great deal of fat is deposited under the skin it is readily accessible to the skin glands. Montgomery states that all starches and sugars have, in process of digestion, to be turned into glucose, and if starch and sugar are taken in excess they are not all absorbed as glucose through the portal system and a great deal is thrown, through the lymphatic and thoracic ducts, into the circulation, without passing through the liver. This sugar, which is not so elaborately digested as that which passes through the liver, he considers, is irritating to the skin glands and causes their increased activity. He also states that many persons with seborrhoeic eczema have enlarged livers, probably from fatty infiltration or excess of stored glycogen. This might cause obstruction to the passage of glucose through the liver, and again the sugar is thrown direct into the circulation without going through the liver. Excess of sugar in the diet may also lead to increased fermentation in the bowel by its constipating effect, and the products of fermentation may lead to sebaceous gland irritation. Sterling pointed out that the milk fats, either as milk, cream, butter, or cheese, are more likely to lead to a deposit of fat in the skin than meat fats. The milk fats are oil emulsions, whereas in meat fats the fat cell has a membrane which has to be digested before the fat is absorbed, and therefore all milk fats are more quickly and easily absorbed than meat fats. Also the fatty secretion of the sebaceous glands in man and the higher animals consists almost entirely of cholesterol esters. These are very little liable to fermentation, and bacteria do not grow well in them. By taking a large quantity of milk fats the sebaceous gland secretes fats which are peculiarly liable to bacterial infection, hence excess of milk fats leads to infections of all kinds in the sebaceous glands. Alcohol, by dilating the skin vessels, may indirectly lead to stimulation of the sebaceous glands.

Apart from foods, there are some conditions which tend to increase the activity of the oil glands.

Heat.—Working in heated atmospheres and strenuous exercise increase the circulation in the skin, and in addition to causing sweating also increase the secretion of oil.

Clothing, etc.—The wearing of heavy clothing, especially flannel, is a well-known contributory cause of seborrhoeic eruptions. Want of cleanliness, irritation of dirt and dust, have also been blamed.

Anaemia.—It is well known that in anaemia the skin is often oily. The anaemia may lead to lowered oxidizing powers of the tissues due to want of sufficient oxygen and therefore to a tendency to the deposit of fat in the tissues.

Toxaemias.—The circulation of toxic substances in the skin may also excite the activity of the oil glands. This is especially the case in intestinal toxaemias and constipation. Acute infections such as the exanthemata and influenza are often followed by seborrhoeic eruptions, or existing lesions are aggravated.

Endocrine Glands.—The possibility of the endocrine glands having an influence on the skin oil secretion must not be lost sight of. Hyperthyroidism makes the skin softer and oilier than normal, but the excessive thyroid secretion may only be the result of some toxaemia which is really the primary cause of the condition.

Diathesis.—Lastly, it has been suggested that seborrhoeic conditions are due to an underlying disposition or diathesis of the patient. Darier describes the seborrhoeic diathesis as "la kérose." He considers it as an anomaly of the skin and not a disease in itself; not a trouble of nutrition of the skin, but a modification of the circulation of the glands and

the keratinization of the epidermis. He believes that individuals with this constitution have a hypertrophy of the sebaceous glands which lasts during their active sexual life. This condition makes their skin liable to the various infections known as seborrhoeids, and later ends with atrophic changes in the skin glands and hairs such as seborrhoeic alopecia.

Barber and Semon in 1918 again drew attention to this theory and support Darier's view that the tendency to seborrhoea occurs in a special type of individual, who is particularly liable to infections and inflammations of a seborrhoeic nature. They think that it may exist in infancy or may appear *de novo* in adults. They suggest that such individuals are suffering from a relative acidosis, and that, owing to insufficient intake of the mono- and di-sodium phosphates and carbonates in the diet, the normal alkaline acid equilibrium is upset. They point out the resemblance of this condition to what Czerny describes as the exudative diathesis in children, where their skins and mucous membranes are specially liable to bacterial infection, so that they suffer from adenoids, enlarged tonsils, glands, and skin infections. Barber and Semon suggest for this dyscrasia the term "status seborrhoeicus."

If this state is in some cases an acquired one due to faulty diet or digestion this theory is not so very far removed from the toxic and food theories. It has a great deal to recommend it, as it fits in with the clinical experience that all the conditions under discussion have something in common in the appearance of the skin and eruptions, and that appearance we are accustomed to describe as seborrhoeic.

Now that we have considered seborrhoeic conditions from the general standpoint, let us take each condition separately, and see wherein they differ in their special etiology.

The True Oily Seborrhoea.

This has already been considered, and the possibility of its being due either to an infection of the sebaceous gland with Sabouraud's bacillus, or to some general condition, either dietetic, toxic, or error of metabolism. But before leaving the subject mention must be made of associated conditions. The association of acne vulgaris with seborrhoea is well known, but as the cause of acne is hardly relevant to the subject under discussion I do not propose to deal with it further. The question as to whether oily seborrhoea leads to alopecia will be considered later under the next heading.

Seborrhoea Capitis.

Next let us consider seborrhoea capitis, and by that is meant the scaly condition of the scalp usually termed dandruff. Under this heading two conditions are usually included: (1) the dry scaly condition called by Sabouraud pityriasis simplex, and (2) the oily scaly condition which Sabouraud calls pityriasis steatodes. This latter condition may later become more inflamed, and lead to the so-called seborrhoeic eczema of the scalp. You are all acquainted with Sabouraud's views that pityriasis simplex is due to the spore of Malassez, and pityriasis steatodes to the spore of Malassez and the *Staphylococcus epidermidis albus*, and seborrhoeic dermatitis to these two organisms together with the seborrhoea bacillus; and with Unna's view that seborrhoeic dermatitis is due to the morococcus (*Staph. epidermidis albus*), so that it is not necessary to elaborate them further. I must say, however, that I have never been able to see any very clear line of distinction between the condition called seborrhoea sicca (pityriasis simplex of Sabouraud) and the more greasy form of the condition where the scales are oily and where there is occasional crust formation. It has always seemed to me that the one is probably only a further stage of the other. They have both this in common, that there is desquamation from the surface of the scalp, and the lesions are obviously inflammatory in nature, and all the evidence, both clinical and microscopic, points to an organismal infection of the skin and glands. Sabouraud ascribed pityriasis simplex to the spores of Malassez, and pityriasis steatodes to the spores and *Staph. albus*, but I do not see why one should go out of one's way to complicate matters when the same cause might operate in both cases. If one assumes that the infecting agent or agents in pityriasis simplex are simply growing in the superficial horny layers of the scalp it would lead to a branny desquamation such as one sees from the growth of the *Microsporon furfur* in pityriasis versicolor. If the condition goes deeper into the epithelium and pilosebaceous follicles of the scalp, then pityriasis steatodes would be produced; and if the infection goes deeper still in the epithelium and corium, then you get more marked inflammation with the production of seborrhoeic dermatitis. In favour of this view is the well-known fact that pityriasis simplex is usually very amenable to treatment, as one would expect in such a superficial infection, whereas the other conditions are much more resistant, probably because, being deeper, it is more difficult to get the therapeutic agent into contact with the infecting agent. The one form often leads on to the other, and usually in the same sequence—first the simple scaliness, then oily scaliness with or without crusting. As Sabouraud's bacillus, the spores of Malassez, and the *Staph. epidermidis albus* are present in all three conditions, and as these organisms have all been found on apparently healthy skin, I do not see that one is able, in the present state of our knowledge, to decide what part each plays in producing the infection.

Before leaving this subject mention must be made of a condition which is generally allowed to be the result of seborrhoea capitis—namely, seborrhoeic alopecia or the common form of baldness. Sabouraud ascribed this to the oily form of seborrhoea, and held that it was due to the prolonged action of the seborrhoea bacillus and its toxins on the hair papillae, but I think it is a general experience that alopecia more often follows the scaly form of seborrhoea capitis, and that, if the hair remains oily, it may turn grey early but usually does not tend to disappear. Darier would include it under his condition of kerosis, and puts it down to an atrophy through fatigue subsequent to the excessive stimulation of the hair and sebaceous glands in the earlier stages of the disease.

The wearing of hard hats, too much brain work, want of exercise, and various other conditions have been suggested as at least predisposing causes to the condition; but, so far as I can find out, no one has suggested what I think is the obvious predisposing cause—namely, wearing the hair short. Alopecia is rare in females and appallingly common in men. The only difference between the way in which men and women treat their hair is that the one wears it short and the other long. The hair was meant to be long, and I take it that the normal stimulus to the growth of the hair is the long hair hanging down the back and blowing in the wind. There is a gentle pull on the hair bulb by the weight of the hair which reflexly through the nervous system keeps the hair in tone and stimulates the circulation in the bulb. If one keeps the hair short, one removes the stimulus to the healthy growth of the hair, and the seborrhoea of the scalp attacks the hair bulb and soon destroys it. Women may have seborrhoea capitis for years, and it practically never leads to the degree of alopecia seen in men. It is a well-recognized fact that all organs which are not used as they were meant to be become weak and liable to the attack of infecting agents. Men also go much more frequently to the hairdresser than do women, and I think it is there that they are probably infected with seborrhoea. The revolving brushes which some hairdressers use are probably particularly to blame, as considerable force is applied and the organisms removed from a previous customer's scalp are vigorously rubbed into that of the next one. This repeated reinfection with a fresh virus from another individual would account for the recurrence of seborrhoea capitis in men in spite of treatment. Most men are probably infected in boyhood, and at first the inflammation of the scalp leads to hyperaemia and stimulation of the growth of the hair. This is the stage at which most youths are at 17 or 18 on leaving school, and the same youth at 24 years or so is beginning to get bald. The only men who have, as a rule, good heads of hair are some of the professional musicians, and that is not because they are musicians but because they wear their hair long.

Seborrhoea Corporis and Seborrhoeic Dermatitis.

This includes the lesions which start as papular, papulovesicular, and vesicular, and end as scaly and crusted areas of a yellowish-red colour, usually spreading peripherally, sometimes clearing up in the centre, and typically seen on the sternal and interscapular regions. There are also the similar inflamed lesions which may occur on the scalp and spread down behind the ears on to the neck, in the axilla, groins, etc. There is no difficulty in assuming the parasitic origin of the more superficial forms of seborrhoea corporis where there are circinate lesions spreading slowly outwards. Here again the same three organisms as occur in seborrhoea capitis may be found, and here again I do not see how one is to decide which organism or combination of organisms is the cause.

The ease with which these cases are cured by local applications, the slight degree of itching present, and the gradual slow spread without marked variation from day to day which one sees in other forms of dermatitis are all in favour of a simple infection. The older theories about flannel, sweating, etc., are probably correct, but these causes are only contributory or predisposing.

But when larger areas are affected, such as the whole scalp, behind ears, axillae, groins, and round the umbilicus, the condition does not look quite so like a parasitic one. In these cases the affected areas are often vesicular, moist and oozing, and crusted, but that is not necessarily against a parasitic cause, as one sees vesicular lesions which ooze in the eczematoid ringworm of the hands and feet, where the lesions are undoubtedly due to a fungus. On the other hand, in the light of recent discoveries in eczema one must take into consideration the possibility of these cases of seborrhoeic dermatitis being a sensitization phenomenon. This applies more particularly to the cases where the eruption, although starting on the typical seborrhoeic areas, spreads more or less acutely and becomes more or less generalized. The eruption flares up and dies down again repeatedly, and is often intensely itchy as in other forms of dermatitis. These cases are also not so amenable to local treatment as the simple circinate ones. Are they cases of sensitization to some protein by internal absorption on which the seborrhoeic virus is implanted on the top, or are they due to the cells of the skin or sebaceous glands becoming sensitized to the protein of the seborrhoeic virus from the outside? This question arises particularly in the infantile dermatitis. Some dermatologists consider these as falling under seborrhoeic dermatitis. The affection of the scalp and face, the spread down behind the ears, the implication of the flexures, and the colour of the eruption in many cases, strongly suggest a seborrhoeic element in them. You will admit that a good deal of evidence has been brought forward to show that such cases in infants are due to sensitization to food proteins. If that be so, then the probability is that the lesions are secondarily infected with the seborrhoeic virus. Then by scratching there is no reason why the child should not sensitize himself to the protein of the virus and so produce a double internal and external sensitization eruption. The same remarks apply equally well to extensive dermatitis in adults, especially in the cases affecting chiefly the flexures with either a scaly seborrhoea of the scalp or an acute dermatitis there. Many of these adult cases get well after some weeks when put to bed and treated with weak sulphur preparations locally and without any alteration in the diet. The local treatment would kill off the virus, and in three or four weeks the dermatitis gradually subsides and does not tend to recur for months or years. Whereas in the ordinary so-called eczemas due to internal sensitization from foods or bacteria the cases tend to relapse repeatedly and local treatment often has very little effect in preventing recurrences. The fact that these cases usually take several weeks or months to recover is in favour of some sensitization being present as against a simple infection. A simple infection would probably get right in two or three weeks. You all remember the stubborn cases of seborrhoeic dermatitis met with during the war. I refer especially to the cases where there was a moist dermatitis of the scalp, ears, eyebrows, and often the beard region, which resisted all local treatment and tended to relapse in spite of treatment. These cases looked like ordinary cases of seborrhoeic dermatitis. Were these cases sensitizations to the seborrhoeic virus or some other organism? Autogenous vaccines were sometimes helpful in these cases, and it is possible that in addition to the seborrhoeic infection these patients were sensitized to the staphylococcus or other organisms in the skin.

As having a bearing in this respect the work of Strickler, Kolmer, and Schamberg is noteworthy. These observers did complement fixation tests with the serums of patients suffering from acne vulgaris, rosacea, and seborrhoeic dermatitis, using the acne bacillus (Unna-Sabouraud type), *B. coli*, and staphylococcus. Of the ten cases of seborrhoeic dermatitis which were examined, 30 per cent. reacted positively with the antigen of *B. acne* and 25 per cent. with the staphylococcal antigen. With the antigen of *B. coli* from acne patients, fixation occurred in 40 per cent. of the serums and in 11 per cent. with the antigen of *B. coli* from normal persons. Several of the cases were mild types of the disease, and that might account for the negative reactions with acne bacillus antigen. These results are interesting from two points of view. First they show the presence of antibodies to skin organisms in the serums of patients with seborrhoeic dermatitis, and support the view that a sensitization to these bacteria has taken place; and secondly, the percentage of cases which gave a positive reaction to *B. coli*, pointing to the possibility of intestinal absorption playing a part in the sensitization of the patient.

The question of a local sensitization of the skin to the seborrhoeic infection is another possibility. It is well known that seborrhoeic eruptions repeatedly recur in the same areas of skin each time. It is possible that these areas of the skin have become sensitive to the seborrhoeic virus, and, therefore, the eruption always recurs in these places.

There is also the type of case of seborrhoeic dermatitis of the head and face in adults where, quite suddenly, for no apparent reason, the whole face becomes affected with an acute erythematous and oedematous dermatitis with swelling of the eyelids, so much so as to suggest erysipelas. Is that an acute streptococcal infection with an organism allied to the erysipelas streptococcus on the top of the ordinary seborrhoeic dermatitis, or is it a sensitization phenomenon? The suddenness with which it comes on and the general appearance of the face suggest a dermatitis venenata. It is possible that the patient is sensitized to the seborrhoeic virus, and by scratching the thin skin of the face rubs in the virus, living or dead, and so produces a local dermatitis venenata.

Pityriasis rubra seborrhoeica must also be mentioned. Are these cases of pityriasis rubra with the seborrhoeic virus implanted on the top of it, or are they general sensitizations to the seborrhoeic virus? The cases of general exfoliative dermatitis which occur from time to time during or soon after the administration of drugs like arsphenamin must be considered as due to sensitization of the skin to these drugs, and similarly cases of seborrhoeic dermatitis going on to pityriasis rubra seborrhoeica may be due to the patient becoming sensitized to the seborrhoeic virus. The eruption spreads much too quickly and too generally to be an extension of the seborrhoeic condition.

The whole question of sensitization is still rather obscure, and further light is required in regard to the subject before any definite statement can be made, but I am convinced that in some at least of the seborrhoeic eruptions sensitization will be found to play a part in their pathogenesis.

BIBLIOGRAPHY.

Audry: Séborrhée-Séborréides, *La Pratique Dermat.*, 1904, tome iv, p. 270.

Barber and Semon: The Etiology and Treatment of Seborrhoeic Eruptions, BRITISH MEDICAL JOURNAL, 1918, ii, p. 245.

Beattie, Wallace: Seborrhoea, *Brit. Journ. Derm.*, 1894, p. 161.

Brocq: Les Eczémas Séborrhéiques ou Séborrhéides, *La presse méd.*, 1897, p. 162.

Brooke, H. G.: The Clinical Relationship of Seborrhoea, *Brit. Journ. Derm.*, 1904, p. 205.

Cumston: Seborrhoea of the Scalp, *New York Med. Journ.*, 1921, p. 156.

Darier: La Kérose, *Ann. de Derm. et Syph.*, 1907, p. 3.

Fox, Colcott: Discussion on Seborrhœa, BRITISH MEDICAL JOURNAL, 1901, ii, p. 855.

Gardiner, F.: Eczema or Seborrhoeic Dermatitis in Children, *Practitioner*, 1920, p. 47.

Hallé and Civatte: The Bacteriology of the Sebaceous Glands, *Ann. de Derm. et Syph.*, 1907 p. 184.

Kuznitsky: Experimentelle und klinische Beiträge zur Frage der Hautdrüsensekretion, *Arch. f. Derm. u. Syph.*, 1913, p. 691.

MacLeod: Seborrhoea and the Seborrhoeides, *Practitioner*, 1904, p. 755.

Montgomery: The Relation of Diets to Seborrhoea, *Journ. Cutan. Dis.*, 1916, p. 829.

Roberts, Leslie: Recent Researches on Seborrhoea and its Consequences, *Brit. Journ. Derm.*, 1897, p. 219.

Sabouraud: *Les Maladies Séborrhéiques*, Paris, 1902.

Southworth: The Predominance of Seborrhoeic Eczema in Early Life, *Arch. Pediat.*, 1920, p. 338.

Strickler, Kolmer, and Schamberg: Complement Fixation in Acne Vulgaris, *Journ. Cutan. Dis.*, 1916, vol. 34, p. 165.

Unna: Das Seborrhoische Ekzem, *Volkmann's Klin. Vorträge*, 1893, p. 669; and *Münch. med. Woch.*, 1921, p. 547.

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THE SEBORRHOEIC STATE.

[*Abstract.*]

IF by seborrhoea we merely mean an apparently excessive secretion of sebum it is extremely difficult, as Dr. Cranston Low has said, to determine the line between the normal and abnormal. The secretion is greater in the black races than in white, and on the whole among Europeans in people of the south than in those of the north. These differences probably partly depend on different climatic conditions, but there is no doubt that excessive greasiness, like excessive dryness, of the skin may be a familial and racial characteristic. One certainly meets with seborrhoeic families, and the Jews as a

race have oily skins; moreover, dark-complexioned persons as a rule have larger and more actively secreting sebaceous glands than blondes.

On the other hand, there is a pathological seborrhoea, and it is when this exists that the various types of eruption to which the epithet "seborrhoeic" is attached are apt to make their appearance. I am convinced that in this condition we are not merely dealing with increased secretion of sebum and sweat, but also with an alteration of their composition, and that it is this as yet not accurately determined change of composition, rather than the increase in the amount secreted, that renders the skin liable to active invasion with micro-organisms which the normal skin is able to resist. In favour of this view is the fact that, although in cases of seborrhoeic dermatitis there is usually obvious hypersecretion of the sebaceous glands, this is not always so. Indeed, one may meet with typical seborrhoeic dermatitis in ichthyotic persons.

There is, in fact, a seborrhoeic state, as Semon and myself insisted from our study of cases met with among the troops, and I may here say that, although further research carried out with Dr. Ryffel and others has caused me to modify my views somewhat, they remain substantially the same as when Semon and I published our paper five years ago. I can, perhaps, best give a vivid picture of this seborrhoeic state by describing the clinical appearances of an acute case; such cases, so common during the war, are still sometimes met with in civilian practice.

The face, scalp, ears, and neck are flushed, giving the patient a congested or plethoric appearance; the sebaceous glands are visibly hyperactive, as also are the sweat glands, so that even in cold weather droplets of sweat may be seen on the forehead, nose, and upper lip. If one tests one of these droplets with litmus paper it will usually be found to be strongly acid, much more so than the visible sweat obtained in normal people, and the high acidity cannot, I think, be due altogether to admixture with sebum. The vermilion borders of the lips are covered with a kind of yellowish scum, and examination of the nose, throat, and nasopharynx will show congestion of the mucous membrane, catarrh, and reddening and hypertrophy of the lymphoid follicles on the posterior pharyngeal wall. The urine is almost invariably highly acid, even if dilute, but occasionally the apparent anomaly occurs that is met with in some cases of diabetes—namely, that the urine may be only slightly acid, neutral, or even faintly alkaline when first examined, but on administering alkalis one finds that for a while it becomes more acid.

In this state the resistance of the skin and mucous membranes to bacterial invasion is diminished; in other words, there is apt to result what may be termed a catarrhal inflammation both of the skin and of the mucous membranes of the nose, nasopharynx, eyelids, conjunctiva, penis, and vulva. The micro-organisms concerned are chiefly the pyogenic cocci, particularly the *Staphylococcus albus* and *aureus;* of less importance are the acne bacillus and the so-called "bottle bacillus," which affect the skin alone. These various organisms produce eruptions whose clinical features differ according to the anatomical position in the skin of the inflammatory changes, the particular organism concerned, its virulence, the resistance of the patient, and the severity and duration of the predisposing seborrhoeic state.

The problem is a difficult one, but I think that, clinically at any rate, we must draw a distinction between seborrhoeic dermatitis, whether occurring in the follicular petaloid form (flannel rash) or as a scaly dermatitis (eczématide), and what I term true seborrhoeic eczema.

My view at present is that we must recognize three distinct conditions: (1) seborrhoeic dermatitis due to infection of the skin with a *Staphylococcus albus* or *aureus;* (2) primary acute seborrhoeic eczema due perhaps to irritation of the skin produced by some acid substance taken up by the epidermal cells; (3) infective eczematoid dermatitis due to sensitization of the skin to pyogenic organisms having occurred, and probably, not always although usually, associated with the seborrhoeic state. This may supervene in either 1 or 2.

The Urine in the Acute Seborrhoeic State.

It is in cases of the acute seborrhoeic state with acute or recurring attacks of seborrhoeic eczema or dermatitis that I have carried out most of my investigations with Drs. Ryffel, Joffe, Grace, and Payne. As I pointed out in the original paper written with Dr. Semon, these cases show increased alkaline tolerance often to an extraordinary degree—that is to say, large doses of alkali may have to be given before the urine becomes alkaline. This fact has been observed in a large number of cases met with since the war, not only among out-patients, in whom, of course, one cannot be absolutely certain that the dose of alkali prescribed has actually been taken, but also among patients in hospital in whom the amount taken is accurately known, and whose urine can be examined daily.

Dr. Ryffel has kindly investigated the urine of several of my cases with a view to determining the ammonia ratio and what may be termed the acid ratio. The former is, of course, the ratio of nitrogen excreted as ammonia to the total nitrogen, and is very high in diabetes; the latter is the acidity of the urine expressed in the same terms as the ammonia ratio. A survey of his figures shows that in these cases both the ammonia ratio and the acid ratio tend to be higher than normal, sometimes very markedly so, and the sum total of the two ratios is almost invariably high.

The question naturally arises whether the alkali in these cases is absorbed. In order to test this point Dr. Payne kindly investigated the faeces of one of my patients who was taking very large doses of alkali, and he found that their mineral content was not above normal figures, showing that absorption had taken place. We may therefore consider it established that in probably the majority of cases of the seborrhoeic state in its severe or acute form there is diminished alkali reserve, often to a very striking degree.

The Blood Sugar in the Seborrhoeic State.

Dr. W. H. Grace, working in my department at Guy's Hospital, carried out some investigations on the sugar content of the blood four or more hours after a meal in certain skin diseases, using the Folin-Wu method of determination, and comparing the results obtained with numerous controls. He found that in cases of acute seborrhoeic eczema or dermatitis, uncomplicated by septic infection in the form of boils, pustules, or abscesses, the sugar content of the blood was not as a rule much above normal, the average figure being 0.116 per cent. In chronic cases, however, in which secondary pyogenic infection had occurred the figure was much higher, averaging about 0.2 per cent., and in some cases reaching 0.28 per cent. It was found that the improvement in the condition of these patients proceeded *pari passu* with the fall in the blood-sugar content.

The Etiology of the Seborrhoeic State.

The chief underlying cause is, in my opinion, faulty digestion and metabolism of carbohydrate, and to a less extent, perhaps, of fat, whereby an excess of organic acid by-products is produced. These are apparently secreted through the sebaceous and sweat glands, causing not only excessive secretion, but also, of course, an alteration in the composition of the sebum and sweat. It is probable, too, that these acid products are taken up by the cells, not only of the glands, but also of the whole epidermis and mucous membranes, and are combined with or dissolved in the fatty substances in which these cells are so rich. At any rate, as Darier suggests in his description of "la kérose," there is presumably an alteration in the chemical composition of the epidermal cells which produces a modification of the process of keratinization, and diminishes their resistance to bacterial infection, probably by affording the bacteria a more suitable medium for active growth.

Once the bacteria—staphylococci and the acne bacillus—become established another factor comes into play—namely, bacterial sensitization or susceptibility, which is not confined merely to the skin, but affects the system as a whole.

The origin and nature of the acids responsible for the diminished alkali reserve and the high ammonia ratio in the seborrhoeic state probably result from the improper digestion and incomplete oxidation of carbohydrate and perhaps fat. The evil effect of an excess of sweets, cakes, pastries, etc., in patients with acne or seborrhoeic eczema and dermatitis is well known clinically, and in my opinion the chief factor in the production of the seborrhoeic state is the taking of an excess of soft carbohydrate foods and their imperfect digestion and oxidation. The diet of a large proportion of our population of both lower and upper classes is hopelessly unphysiological, especially in large towns. The tendency is to take an undue proportion of soft, concentrated carbohydrate food, which cannot be masticated and which is far in excess of the needs of a person living a sedentary life, very often in overheated, imperfectly oxygenated surroundings. On the other hand, fresh green vegetables and fruit, which supply a maximum of alkaline salts and a minimum of nutriment, are taken sparingly or not at all by many people. Some, too, not only take an excess of carbohydrate but devour large quantities of meat as well. I am in the habit now of asking

many of my patients to write down everything they eat during a period of three or four days, and their resulting diet sheets are often amazing.

Treatment.

I will conclude by considering the treatment of the seborrhoeic state, the principles of which are based on the etiological factors I have suggested. In its acute form, which may be associated with primary seborrhoeic eczema, the administration of large doses of alkali without any local treatment is specific. When sufficient alkali has been given the flushed appearance and the hypersecretion of the sebaceous and sweat glands disappear, and the eczema, if present, dries up. Moreover, the patients confess to a feeling of well-being, their appetite returns, and the sensation of heat and itching in the skin is lost. In the chronic form of the seborrhoeic state, in which bacterial infection has become established, producing clinical seborrhoeic dermatitis, acne, sycosis, etc., the problem is not so simple, for bacterial sensitivity has become a superadded factor, and is often the more important. Treatment then consists in regulating the diet as one would in a mild case of diabetes, in insisting on regular out-of-door exercise, in ensuring a sufficient intake of alkaline salts by giving plenty of fresh green vegetables and fruit, and, if necessary, by administering sodium bicarbonate before meals; in removing any septic condition of the mouth and throat, or a focus such as a chronically inflamed appendix; in prescribing full doses of hydrochloric acid after meals if there is hypochlorhydria or achlorhydria, as there is in most cases of rosacea, and in giving an autogenous vaccine prepared from the infecting organisms. Local treatment in these cases is, of course, of great importance, and its rationale is to render the skin dry by means of sulphur, salicylic acid, resorcin, the mercurials, and the *x* rays. But unless the diet, the digestion, and the mode of life of the patient are regulated, relapse is almost certain to occur when the local treatment is discontinued. With regard to vaccine treatment, I am aware that many observers of greater experience than my own are unconvinced of its value. It is probable that chemotherapy by preparations such as collosol manganese and stannoxyl may eventually take its place, but till then I shall remain one of its strongest advocates. It must, however, be controlled by serological tests, it must be given intelligently, and it must be combined with rational treatment of the underlying pathological state which has led to the lack of resistance to the infecting bacteria. I believe that the unfavourable opinion that has been formed by many dermatologists of the value of vaccine therapy in infective conditions of the skin is due partly to the use of stock vaccines, which I have now entirely abandoned; partly to the fact that the specificity of the organisms isolated is not determined by serological tests; that the dosage is injudiciously planned—for it is easy to make a condition worse with vaccines—and that the injections are not persevered with long enough; and partly that the underlying factors, which have lowered the patient's resistance to infection, are neglected. In some chronic cases of infective eczematoid dermatitis and of generalized seborrhoeic dermatitis, which are often accompanied by boils and hydradenitis, I have found all treatment unavailing until an autogenous staphylococcal vaccine was given, whereupon steady improvement began and continued until a cure was obtained. In some of these stock vaccine had been given without effect. In spite of all arguments to the contrary, my clinical experience has convinced me of the specificity of vaccine therapy, and, until we can find infallible methods of chemotherapy against the various types of infecting organisms, I believe that specific vaccination will continue to be the method of choice rather than non-specific protein therapy.

DISCUSSION.

Dr. R. W. MACKENNA (Liverpool) expressed the pleasure the Section had derived from listening to the two opening papers. He agreed with the remarks made by the President in introducing the discussion that the results of the frontal attack upon the question of seborrhoea and the seborrhoeides had been somewhat unproductive of definite results; but he believed that the flanking attack made by Dr. Barber and his colleagues on the question, along the lines of biochemistry, was likely to be much more fruitful. He paid a compliment to the work done by Dr. Barber during recent years—work which was likely to leave a permanent mark on dermatology. With regard to seborrhoea, he was of opinion that the hyperactivity of the sebaceous glands, which was a necessary antecedent to all the skin lesions classed as seborrhoeic dermatitis and the seborrhoeides, depended on some stimulation by the secretions of the endocrine glands. But the oversecretion alone was not everything. There was some other factor, and that factor was probably a micro-organism of some sort. When he read Dr. Barber's and Dr. Semon's original paper on the seborrhoeic state he was of opinion that they had made an illogical and unwarranted deduction from the facts before them; for it was unsound reasoning to conclude that over-elimination of acid in the urine told us anything about the alkali reserve in the body. Overacidity of the urine might mean simply overproduction of acid, and equally rapid elimination in order to keep the alkali reserve normal. Dr. Barber's paper that day, however, embodying the results of his later researches, proved that his (Dr. Barber's) original deduction was correct—namely, that in seborrhoeic conditions there was a definite lowering of the alkali reserve, and, coincidently, an increased alkali tolerance. He did not agree with Dr. Cranston Low's theory as to the retention of their hair by women. It depended, he believed, on endocrinous secretions. As Sabouraud long ago pointed out, eunuchs never go bald. With regard to protein sensitization in the skin he agreed with Drs. Low and Barber that the possibility of its place as a factor in the production of seborrhoeic dermatitis should not be forgotten; but he thought one should go warily in using protein sensitization as an explanation of all dermatological conditions of obscure cause.

Dr. KENNETH WILLS (Bristol) said that emphasis should be laid upon the infective nature of pityriasis capitis. He had found from clinical observation that eczema seborrhoicum ran in families, and that it was directly conveyed from parent or nurse to children. It was caught on the parts of the skin exposed, and might spread from those inoculation points. The primary lesion was a plugging of the follicles, followed by circumfollicular erythema with itching. Subsequently rubbing and scratching led to "eczematization." The same observations might be made in adults, where the cocci were conveyed from a scurfy scalp by scratching. Thus an insect bite or other simple itching lesion might become infected with eczema seborrhoicum.

Dr. DOUGLAS HEATH (Birmingham) said that he also had noted that large doses of alkalis improved acute seborrhoeic conditions, and he had also found that sulphur lotion was much improved by being made up in alkaline solution. Olive oil, although a popular remedy, he considered was very deleterious to seborrhoea. He had obtained good results in the treatment of acne from the use of a mixed vaccine.

Dr. A. M. H. GRAY (London) asked the meeting to define what was meant by seborrhoea, seborrhoeic dermatitis, and seborrhoeic eczema. He considered seborrhoea to be a mere excessive secretion of the sebaceous glands; he excluded from it conditions where there was any inflammation whatever. Seborrhoeic dermatitis he called those cases where there was some inflammation, the most typical of which were the cases of circinate and figurate dermatitis in the centre of the chest and back, also pityriasiform conditions of the scalp. In both of these bacteria, but of doubtful nature, were concerned. He refused to associate premature alopecia with the barber's shop and local infection; in the present state of our ignorance of the endocrine glands he preferred to stress the hereditary factor in these cases.

Dr. NORMAN WALKER (Edinburgh) believed, on the contrary, that the local cause was responsible for the loss of hair. He believed in frequent washing of the scalp as a preventive of baldness, and in the treatment of the scalp as the key of all the treatment of seborrhoeic conditions, and above all of rosacea. He also emphasized the importance of treating oily seborrhoea of the scalp when treating acne vulgaris. He referred to the importance of dermatoses, especially scabies, in war.

Dr. O'DONOVAN (London) referred to the frequency of hyperglycaemia in connexion with inveterate sycosis, but found that the exhibition of alkalis was of no therapeutic value. He had got the best results in bad sycosis from painting with the liquid acid mercury nitrate, which was painful but effective.

Dr. F. ANDERSON MURRAY (Glasgow) said that the question of treatment of seborrhoea and seborrhoeic dermatitis was especially interesting; he thought that insufficient attention

had been paid to the preventive treatment of seborrhoea. He thought that the scalp was frequently infected in childhood, and should be treated accordingly. Later in life the condition frequently was a question of occupation. Diet was also of considerable importance: patients prone to the condition should be sparing in the use of starch and sugar.

Dr. W. H. BROWN (Glasgow) referred to two cases in which gastric analysis showed great hyperacidity. Dr. LANCASHIRE (Manchester) also emphasized the importance of the preventive treatment against seborrhoea during childhood. He said that in local treatment he got the best results from sulphur in the form of ointments. He had been disappointed with colloidal sulphur.

Dr. J. GOODWIN TOMKINSON (Glasgow), in view of the conflicting theories as to the causation of seborrhoea, hesitated to dogmatize. He was, however, strongly inclined to accord a very important part to the microbacillus of Sabouraud. It was known that in some instances it might be present in the skin without causing any apparent modification in secretion, but its constant presence in teeming numbers in the cocoon was, to say the least, suggestive. This raised the question of contributing factors—factors which elicited or determined the pathogenic faculties of the microbacillus. On the one hand, these might be internal, on the other local, due possibly to the symbiosis of two or more members of the cutaneous flora. Among the internal causes one connected with the maximal manifestations of seborrhoea was puberty, which was associated with great glandular activity. Again, there were other adventitious internal conditions known to aggravate the eruption of seborrhoeides. It was well known that overwork sometimes determined an outbreak of seborrhoeic dermatitis. He had had a striking instance of this in his own practice about two years since. A medical student, long the subject of seborrhoeic dermatitis in the presternal region, had had the misfortune to fail in his final examination and had been unduly depressed thereby. He had also been unduly addicted to sweetmeats. When seen by the speaker he was confined to bed and feverish, and his skin was in a condition of generalized exfoliative dermatitis.

The President (Dr. LESLIE ROBERTS, Liverpool) said that the conclusion arrived at as to the nature of seborrhoea depended on the point of view taken by the investigator. Clinical investigation carried them but a little way. There were two main lines of promising research: (1) that of the comparative microscopical anatomy of the vertebrata; (2) the biochemical method. The comparative anatomical method brought to light a remarkable correlation between the pilogenetic and sebaceous functions of the follicles. In the fulfilment of both these functions cells were cast off bodily. The follicle was, in fact, a primitive moulting organ, in the exercise of which function the clinical manifestations were strikingly different—namely, alopecia or baldness, or the arrested development of hair, as in the lanugo hairs, coupled with an increased output of fat-laden epithelial cells. Alopecia areata was the most complete example of the seborrhoeic or moulting process. In birds and reptiles in which the keratin-laden cells amounted to an exoskeleton, the sebaceous glands were either entirely absent or were gathered together into a single gland mass situated at the root of the tail of most birds, or were remarkable for the paucity of their numbers. The same correlation was observed in the human skin in the palms and soles, where some approach to a horny exoskeleton was found. If the true fat glands of the skin were looked upon as organs for the secretion of fatty acids, then the true fat glands of the mammalian skin were the sweat glands which found their highest development in the mammalian milk glands. The biochemical line of research would probably carry them farther into the heart of the subject than the anatomical. Dr. Barber and his collaborators had done useful work on those lines. They had ascertained experimentally certain facts which confirmed the opinion of Darier and others that the moulting process of keratin and fat cells was associated, and indeed was the outward manifestation of a chemical condition of the tissue fluids which seemed to consist, in part at least, in a lowering of the basal salts. Dr. Barber and Dr. Semon had demonstrated an increased alkaline tolerance in severe cases of seborrhoeic dermatitis. The interpretation of the biochemical data was, however, extremely difficult, and before this observation could be definitely established as a general truth it was necessary to ascertain whether lowering of tissue alkalinity and increased acidity of the urine was not a natural accompaniment of all cutaneous and visceral inflammations. It had been shown to accompany erysipelas and pneumonia, and the question arose as to whether the hypo-alkalinity of the tissue fluids was not due to the withdrawal of basic salts from the blood, and in reality had no connexion with the seborrhoeic process itself. This must be left for future investigation to decide.

The question of blood sugar was one of great practical importance in relation to inflammation of both the skin and the viscera. That its presence in the inflamed tissues was inimical to recovery was well established, but it was necessary to differentiate between the saccharides. Taking glucose as a typical monosaccharide, it had been shown by Folin and Berglund that the ingestion of 200 grams of pure glucose did not raise the level of the blood sugar above the renal threshold of glucose secretion. There was reason for believing, as Folin and Berglund had pointed out, that the absorption and excretion of foreign unsuitable carbohydrate material present in grain, vegetables, and fruit was of daily occurrence; and to this must be added the decomposition products due to cooking, canning, and baking of the food. There was, so far as we knew, no mechanism in the digestive tract for preventing the absorption of these soluble but incompletely digested carbohydrates. These authors further suggested that foreign carbohydrates played an important part in the etiology of many obscure skin and joint troubles. But we were going farther than the evidence warranted if we assumed that they were essentially connected with the seborrhoeic or moulting process. Turning to the clinical side of the seborrhoeic dermatoses, he was bound to admit that the group was vague and ill defined. All inflammatory reactions which were frankly connected with the moulting process could be rightly called seborrhoeic. These included the dry, discoid or circinate, infiltrated, hyperaemic patches met with on the front of the chest and on the back. The eczematization of the scalp coexisting with the loss of hair and increased output of sebaceous material must also be included in the group, and this form in many cases showed a tendency to spread downwards behind the ears, and to appear in the bends of the elbows and groins. In the war forms of severe "seborrhoeic dermatitis" described by Dr. Barber it was probable that an emotional factor played a considerable part in determining the form and distribution of the disease.

Dr. CRANSTON LOW, in his reply, again emphasized the necessity for a revision of the nomenclature, and said that there would always be confusion until the term "seborrhoea" was only used in its true sense. He referred again to the cutting of the hair as the cause of common baldness. In races such as the Cingalese, where the men all wore their hair long, baldness was practically unknown. Dr. Low also sounded a note of warning with regard to the inferences to be drawn from the biochemical changes in the blood and urine in seborrhoeic conditions, and suggested that these changes might be the result and not the cause of the skin condition.

Memoranda:

MEDICAL, SURGICAL, OBSTETRICAL.

MELANOTIC SARCOMA OF CHOROID AND OVARY.

MELANOTIC sarcoma appears to be accepted as the most virulent form of malignant tumour on account of the early date at which it forms secondary deposits in other parts of the body. For this reason, and because the literature on the subject is meagre in quantity and lacking in details, I venture to describe a case which shows some interesting features.

Mrs. M. is now 38 years of age and, with the exception of minor ailments incidental to an overstrung nervous temperament, and severe eclampsia previous to the birth of her second baby seven or eight years ago, has enjoyed fairly good health. She has worn an artificial right eye for many years, her explanation being that the sight failed in her right eye in August, 1906, and following a course of medical treatment (during which sixty bottles of medicine were consumed) the eye was removed on April 24th, 1907, for a "small tumour"—she being then 23 years of age.

In September, 1921, she consulted me about a small lump she had noticed under the skin of the right upper arm near the anterior border of the deltoid muscle. The skin being thin and fair the tumour showed almost black beneath. I excised a dead black lobulated growth the size of a horse bean. On microscopical

examination it proved to be a typical spindle-celled melanotic sarcoma, melanin permeating the cells as well as being intracellular. In view of the fact that her eye had been removed fourteen years previously I obtained from Mr. Collier Green, who performed the operation, the information that there was a small tumour of the choroid close to the ciliary region, which on examination was pronounced to be a pigmented spindle-celled sarcoma. I thereupon made a complete examination of the patient and found another subcutaneous tumour in the middle of the back near the fifth or sixth dorsal spine, and *per vaginam* a tumour the size of an orange—whether uterine or ovarian I could not determine. Mr. H. T. Hicks, gynaecological surgeon, was called in consultation, and advised early removal of the tumour. This was carried out on October 3rd. The subcutaneous growth in the back was first excised and was found to be similar in all respects to the one I removed from the arm. The pelvic tumour was ovarian (left), the size of a large orange, glistening black and lobulated, solid with the exception of a small cyst containing dirty black fluid. The section was dead black. The right ovary was apparently healthy, but it was deemed advisable to remove it.

The pelvic glands did not appear to be involved and other organs were apparently normal. On microscopical examination both growths proved to be spindle-celled melanotic sarcomata.

Recovery was complicated by cerebral symptoms suggesting thrombosis; the patient lay in a restless and semi-unconscious state for two or three days. This slowly passed off, and in a few weeks' time she had more than regained her usual health; in fact, all her friends tell her she has never looked so well. She has put on weight and has a nice rosy colour in her cheeks. No sign of recurrence could be found at a complete examination on March 29th last; this I am going to repeat again shortly.

The interesting features are, first, the long time which has elapsed between the first onset of the disease (the primary growth of the choroid) and the appearance of secondary growths in the skin and ovary—namely, fourteen years; this is all the more striking as failure of sight was noticed eight months prior to the enucleation of the eye. Secondly, the absence of lymphatic gland involvement. Thirdly, the absence of any further trouble nine months after operation.

Derby. H. G. W. DAWSON, M.B., Ch.B.

LUPUS ERYTHEMATOSUS TREATED BY DIATHERMY.

FROM time to time cases of lupus erythematosus treated by diathermy are recorded, but in all cases fulguration appears to be the method adopted. As a different method was employed in the following case with a satisfactory result, the notes may prove of interest.

Mrs. S., aged 35, was sent to me by Dr. W. B. Mackay, C.M.G., of Berwick, on February 25th, 1921, who asked me to try the effect of diathermy. About five years previously she had noticed a small red itchy spot on her nose during the winter. It disappeared during the warm weather, but reappeared the following winter, and since then had gradually become larger. Dr. Mackay was then away serving in the army, but she received treatment without relief, and was seen by a well-known dermatologist in another town, who diagnosed lupus erythematosus, and prescribed for the condition, which, however, proved obstinate.

When I saw her first the lower two-thirds of the nose were affected. Gauze dipped in saline was placed over the nose, and a soft metal pad moulded over it. The current was then turned on, and gradually increased to as much as she could bear, the idea being to get a vigorous reaction, but to stop short of burning. She was told to say when the heat became severe, but in spite of this and of frequent inspections a small burn appeared. The current used was 500 to 600 milliampères. Owing to various causes she was unable to return till May 5th, when there was a considerable improvement in the condition. The burn, which had been superficial, had healed with very little scarring. On this occasion the current was kept at about 300 milliampères, and switched off whenever she complained of the heat. Improvement again occurred. After a third application the nose was normal in appearance, and the patient, who previously was very unhappy about her disfigurement and had avoided mixing with others, was much brighter and was going about much more.

About the end of November a very small red itchy point reappeared. This was again treated three times, and from the middle of January was not visible, but two further applications were made, one in February and one in March. Two months later she again came to see me as she felt a little irritation, and though nothing was visible a precautionary application was given. I saw her again on July 30th, when the nose appeared in every way normal and the scar scarcely visible. I hear that she is free from any return at present.

Each application took from twenty-five to thirty minutes, and the nose was inspected frequently lest any burning occurred. After each treatment the nose was very red and swollen for a few days.

I have not seen any mention of this method being used before, but think that applications at shorter intervals than it was possible to give in this case would be better.

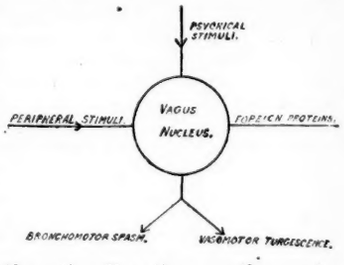
W. J. HARRISON, M.B., M.R.C.S.,
Surgeon, Ear, Nose and Throat Hospital, Newcastle-on-Tyne.

Reports of Societies.

THE PROBLEMS OF ASTHMA.

THE Section of Therapeutics and Pharmacology of the Royal Society of Medicine held its first meeting of the session on October 10th.

Dr. W. LANGDON BROWN, in his presidential address on the problems of asthma, said that in pursuance of his conception of the functions of the Section he had chosen as his subject one which had both clinical and experimental aspects. The problems of asthma were for years most baffling, but recent work had cleared the ground and defined the issues. Those problems had now revealed themselves as involving subjects of most topical interest—psychotherapy, sensitiveness to foreign proteins, vagotonia, and endocrine balance. The treatment of asthma summarized in itself, as it were, the chief trends of modern therapy. But there had been a tendency on the part of enthusiastic workers in some one of those fields to neglect due consideration of the other aspects: successful treatment implied recognition of them all. It was always a dangerous thing to try to express a disease in a formula. Nevertheless, he attempted to do so for asthma not long ago. Hurst had recently improved upon that formula, and he accepted his emendation gladly. Hurst had defined asthma as due to an unstable or irritable condition of the bronchomotor portion of the vagus nucleus, which caused it to react unduly to psychical or peripheral stimuli, or to foreign proteins in the circulating blood. He (the President) thought that this could still be modified with advantage by including the obvious vasomotor turgescence which accompanied the bronchomotor spasm, as it was always present and was sometimes the most striking feature of an attack, just as it was in the closely allied condition of hay fever. Moreover, it was carried out by the same vagal mechanism. Thus modified, the formula could be expressed diagrammatically by the accompanying figure.



He would first consider the stimuli acting on the centre and then some features of its response.

Psychical Stimuli.—That asthma often occurred in neuropathic families and that asthmatics were unduly suggestible were well-known facts. The paroxysm excited by an artificial flower figured in every textbook. But it was extraordinary to witness the almost instantaneous relief that might follow a hypodermic injection of distilled water into a patient who was anticipating one of adrenaline, when that was known to be efficacious. It made him wonder how much of the success of single minim doses of adrenaline was due to suggestion and how much to the drug. At any rate, it explained why the asthma curer who had profound belief in the efficacy of his treatment found increasing justification for that belief; for his confidence aroused responsive confidence in his patient. But what was not so clearly recognized was that the effective stimulus often arose from a psychic conflict. Such a conflict might express itself at the psychic level of the nervous system as an obsession or a phobia, and at the sensorimotor level as a paralysis, a tic, a contracture, or an anaesthesia; while if it sank deeper to the visceral level it might express itself as glycosuria, exophthalmic goitre, or asthma. Naturally, other influences would help to decide in which of those ways the psychic trauma revealed itself. Thus one sufferer from a psychic conflict who had to face a hostile environment might escape through an hysterical paralysis, but another who had to face the internal disorder produced by sensitiveness to foreign proteins was more likely to develop asthma.

Peripheral Stimuli.—Dr. Langdon Brown said he had little to add to the common stock on this subject. The influence of eyestrain, hay fever, and other nasal troubles, sinus infection, gastric and intestinal disturbances, and of uterine disorders was well recognized. But he would like to call special attention to enlarged bronchial glands, especially in children, and to suggest that that might explain the liability of healed tuberculosis to excite asthma in susceptible subjects. Interesting observations by Baccarini showed that peripheral irritation of the pleura, such as paracentesis, might excite an attack of asthma. In the epileptic

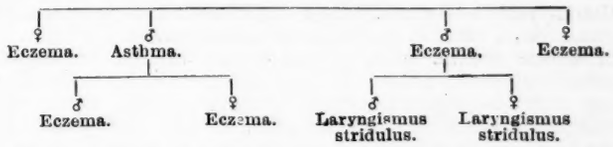
a fit could be similarly produced, which led Baccarini to compare bronchial spasm to a localized epilepsy. Other points of resemblance between epilepsy and asthma would readily occur to one.

Foreign Proteins.—Of recent years great attention had been paid to this factor. Morley Roberts had made the profound remark that immunity was assimilation. There was one flesh of birds and another of beasts. From the welter of amino-acids which resulted from the disintegration of food proteins each animal had to build up its own characteristic and specific tissues. Specificity was chemical as well as morphological. To some foreign proteins a man was naturally immune—that is, he could assimilate them automatically; to others he acquired an immunity—that is, he learnt to assimilate them. But to some foreign proteins immunity was neither congenital nor acquired. The tissues continued to resent the intrusion of such proteins: they would not assimilate them. Such proteins excited anaphylaxis in varying degrees. Richet had defined anaphylaxis as the last stand of the race against adulteration of its protoplasm. In extreme degrees anaphylactic shock was fatal because assimilation would mean too profound an alteration of bodily structure. In less degrees anaphylaxis declared itself in violent attempts to get rid of the foreign invader. As Drury had expressed it, the toxic idiopathies were conservative and self-repairing mechanisms under parasympathetic control.

The Influence of the Parasympathetic.—The great function of the parasympathetic might be defined as promoting the assimilation of suitable and the rejection of unsuitable material. Thus it started the secretory and muscular processes of digestion, while it emptied the rectum and bladder, and could reject food by vomiting. The excitation of bronchial catarrh and cough was in like manner a way of ridding the body of unsuitable material, and it is interesting to recall here that drugs which were expectorant in small doses were emetic in larger ones. The similarity of the parasympathetic action on the alimentary and respiratory tracts was not surprising when one remembered that the latter developed as an outgrowth from the former. The vagal factor in normal respiration might indeed be defined as a mechanism for ensuring the alternate taking in of assimilable oxygen and the rejection of unnecessary CO_2. Confronted with an irrespirable gas, the vagus checked the entrance of this to the lungs by laryngeal spasm. But he did not think it possible to explain in that way bronchomotor spasm, which, while tending to interfere with elimination, did not prevent the entrance of unsuitable material. That, he thought, was an example of the exaggerated response so typical of pathological states. The undue irritability of the vagus centre set up such a strong efferent impulse that it was not confined to the vasomotor channels, but overflowed along the bronchomotor fibres. Without accepting the whole of Eppinger and Hess's theories, one might agree that the asthmatic was a vagotonic, and was liable to other manifestations of vagotonia. He recently had a curious instance of this in a man who suffered from both asthma and gastric ulcer, and who had found out for himself that he could relieve the pain of his ulcer by smoking a stramonium cigarette. And he (Dr. Langdon Brown) would agree with the description which had been given of a form of mucous colitis as "asthma of the colon."

The Sympathetic and Endocrine Balance.—But vagotonia might be relative as well as absolute. In other words, it might well be that an overacting vagus was due to diminished action of the antagonizing sympathetic. One of the ways in which the sympathetic carried out its defensive function was by activating those endocrine glands which flooded the blood with sugar, thus increasing metabolism and exciting a febrile response. Lest this excess of sugar should be wasted by overflowing into the urine the threshold of the kidney was raised in fever. But the febrile reponse was not the only way in which immunity was acquired. One of the greatest gaps in present knowledge was our almost total ignorance of the part played by the endocrine glands in immunity. Beyond the fact that thyroid extract might increase the yield of amboceptors, following the injection of an antigen, hardly anything was known on this point. Yet he felt that further research would show that just as anaphylaxis was associated with vagotonia the opposite condition of sympathicotonia was related to immunity through the endocrine system. Not infrequently hypo-adrenalinism followed or accompanied anaphylaxis, and certainly adrenaline had a valuable action in anaphylactic shock. It had a similar effect in some cases of urticaria. Hurst considered that the toxic idiopathies, including asthma, produced their effect, at any rate in part, by depressing adrenal activity, and he believed that the influence of fatigue in inducing the asthmatic paroxysm was capable of a similar explanation. Morley Roberts suggested that so might the apprehensiveness so common in asthmatics be a factor. The overactivity of the broncho-constrictor fibres of the vagus was kept in check by the broncho-dilator action of the sympathetic, which the secretion of adrenaline would stimulate. Hence the effect of an injection of adrenaline, or of sudden fright in checking an attack of asthma. Asphyxia had a similar action and produced the same effect, and was thus, no doubt, a factor in bringing the attack to an end without a fatal issue. Stewart and Rogoff, it was true, denied this emergency action of adrenaline; but Cannon's recent experiments had disposed of much of their purely destructive criticism. The liability of asthma to come on during sleep, when the parasympathetic gained the upper hand, further pointed to the influence of disturbed balance between the two great branches of the visceral nervous system. The reinforcement of the adrenaline effect by pituitrin might be held to point in the same direction. The glands which co-operated with the sympathetic appeared generally to help in checking the asthmatic paroxysm. The stimulating effect of the gonads in the sympathetic-endocrine group might account for the influence of uterine disease in exciting asthma. It must be admitted, however, that it was difficult on this hypothesis to explain why asthma was liable to appear at puberty and to be aggravated by pregnancy, when the thyroid and pituitary were stimulated, unless it was that some asthmatics had a gonadal deficiency which declared itself in that way. Certainly gonadal deficiency might be associated with thymic asthma, which was comprehensible, since the thymus was an infantile organ, apparently antagonistic to the gonads. In general terms, it would appear that when the endocrine balance swayed in the direction of the vagus, asthma was likely to occur in susceptible subjects; when the balance was redressed in favour of the sympathetic, attacks might be cut short or prevented.

Asthma, Anaphylaxis, and the Skin.—The skin undoubtedly has excretory functions. He recently had a severe and ultimately fatal case of uraemia, which first manifested itself by a pronounced toxic erythema. Urea was greatly increased both in the blood and cerebro-spinal fluid, while in the latter other amines were demonstrated to the extent of 0.24 per cent. The rashes of the exanthemata had often been compared with that produced by serum in anaphylactic subjects. This idea of an excretory mechanism found expression in the popular notion that it was good to "get the rash out." The toxin which could not be assimilated—that is, to which the body was not immune—must be got rid of somehow, and at any rate removed from the vital structures. Here it was interesting to note the occasional alternation of attacks of eczema and asthma. The following family tree which came under his own notice was a striking example of this alternation in different members of the family.



The condition of laryngismus stridulus was recognized as the infantile equivalent of asthma. It was further of interest to recall that both in skin diseases and in asthma eosinophilia was a usual feature of the blood picture. As an attack of asthma went on and expectoration became more abundant the eosinophiles passed into the sputa. He should regard the skin reaction of an asthmatic to foreign proteins as an attempt to wash out the offending non-assimilable material from the skin. He was proceeding to look for the occurrence of eosinophilia in the urticarial wheals produced by this test when Dr. Mackenzie Wallis informed him that he had already observed it. He thought that that all accorded with the observations of Kanthack and Hardy, thirty years ago, on the part played by eosinophilia in bacterial infections and intestinal digestion, which had passed into undeserved oblivion.

A Rational Basis for Treatment.

Such considerations as these helped to provide a rational explanation for some methods of treatment which had been empirically used in the past. He hoped also that they might help to co-ordinate the views of those working at different aspects of the problem, and to emphasize the necessity for all-round assault on every case of asthma from those various aspects. For some, asthma was merely a psychological problem; but that was to close one's eyes to the toxic factor. For others, the toxic element was the primary thing, the nervous factor being merely the failure of the nervous system to co-operate successfully in getting rid of the toxin. That view had been well expressed by Morley Roberts thus:

"Why is there a spasm? What *is* the spasm but violent overacting surface tension pulling every cell in the small bronchi and alveolar spaces into its least form, as it tries to squeeze out irritants—tries to defaecate, so to speak. The sympathetic and

parasympathetic rush in to aid. There is a tendency to overrate nervous action. Cells live their lives, have their disasters, even without nervous interference except in stress, as they did in the beginning."

But that view was not capable of explaining the influence of suggestion in actually producing a paroxysm in the absence of the exciting toxin. The suggestibility of the patient should be utilized to help him confidently to expect a cure. The effect of climate might often be due to suggestion. An asthmatic went to a place and had an attack; therefore, each time he went there he expected to have one, and had it. The converse was also true. To point that out in the former instance might help him to avoid repetition. But search must also be made for deeper-seated psychological troubles. Naturally, sources of peripheral irritation must be eliminated; that included avoidance of late suppers and cold bedrooms. The skin reactions, in his experience, were very helpful in determining the foreign proteins to which the patient was sensitive. Not only in asthma should that be tried; it was worth trying in any vagotonic with toxaemic symptoms.

He recently had under his care a patient who was emaciated and profoundly depressed, but whose chief objective trouble was a painful swelling of the tongue and lips. Dr. Mackenzie Wallis tested his skin reactions and found him highly sensitive to cereals. In addition to other simple measures, cereals were removed from the diet, and he was allowed many things which had formerly been prohibited in the belief that he had gout. The effect was dramatic; in one month he put on 16 lb. in weight. Only a day ago he received a letter from his medical man, in the course of which he said, "I think his almost daily progress has been one of the most marked and interesting cases I have had under my observation for a long time. Socially he is entirely altered . . . bright and entertaining, and his old-fashioned courtesy has returned. In fact, he is so far a complete revolution and revelation."

An experience like that made one think seriously of the toxic factors which must often be overlooked in cases diagnosed as neurasthenia or "functional." Where the offending protein could not thus be eliminated, one should consider what methods of desensitization were feasible. He would like to add that in his experience it was most important to exclude the syphilitic toxin as the offending protein. The universal belief in the efficacy of the belladonna group in asthma found its justification in the paralysing action of such drugs on the parasympathetic endings, just as the fillip given to the sympathetic by adrenaline or cocaine helped to redress the balance in another way. According to some the influence of iodide was mainly to activate the thyroid; if that was so the benefit of th s drug in asthma was comprehensible, since the thyroid secretion lowered the threshold to sympathetic stimulation. But so far there was no explanation of the way in which arsenic acted in this disease, though certainly it often seemed to help. The importance of doing everything one could to restore an impaired endocrine balance was, he thought, undoubted. In conclusion, an all-round attack on the problems of asthma seemed to him to afford the best chance of relief, especially if carried out before structural changes such as emphysema had occurred. After that a vicious circle was set up which it was hard to break.

Discussion.

Dr. FRANK COKE thought that medical men were in great danger, in dealing with asthmatic cases, if they looked upon any of them as being certainly cases which were psychic and possibly neurasthenic, and so forth. A gentleman came to him from one of the nerve specialists, and it was rather a question whether he was not going to have general paralysis; he was very peculiar in a great many ways. On his skin being tested he was found to be sensitive to nothing else but potatoes. The removal of them from the diet had an effect exactly like that in the case mentioned by the President; the patient was now perfectly normal and contented in every way. The question of locality and so forth was important. A small boy who had asthma at home was sent to him. He went to stay with five different aunts and uncles; with three of them he had asthma badly, and with two of them he was perfectly free. At places which were particularly healthy he had asthma very badly. On the boy being tested he was found to be sensitive to dog hair. Another case was that of a lady who was perfectly free from asthma for six months until she went to the seaside. On the first night she developed asthma. It was found that the quilt over her bed was covered with dog hair; that was obviously the cause of her asthma; it was taken away and the asthma went immediately. The influence of the proteins was astonishing. A negro who gave only a + 1 reaction to hen feathers became free from asthma, but had it again as soon as he moved his lodgings and slept on a feather pillow. As to the balance between the sympathetic and the vagus, that subject was too deep to be touched upon on the present occasion; but he thought every case should be tested with the proteins.

Dr. H. CRICHTON MILLER wished to underline one point dealt with by the President. In dealing with asthma one was dealing on the endocrine side so notably with adrenal instability, and on the emotional side one was dealing equally notably with fear complexes. It was more especially the fear complexes that on the emotional side contributed to the asthmatic condition. The connexion between those two sides seemed to him too important to be forgotten. The adrenal being a gland that was naturally associated with the whole fear mechanism all through the animal kingdom, the asthmatic paroxysm was attributable in some degree to an inability to cope with fear. That was putting the matter very widely indeed. How far any sensitization, any failure to immunize the organism against poison, might or might not render the individual less capable of dealing with fear, was a point which had to be cleared up in each individual case by such tests as those described by the previous speaker; but even then the medical man would still have in his hands an individual whose capacity for dealing with all the fear emotions was inadequate. That was one reason why mucous colitis and asthma were associated in the same kind of people. In both there was the same series of emotional failures, and also, he thought, the same series of endocrine failures.

The PRESIDENT, in answer to a question, said that the British habit of sitting in front of a fire during the evening and getting thoroughly baked and then going up to a cold bedroom ought to be avoided by an asthmatic. The great changes of temperature so common in houses without some kind of central heating were harmful.

Dr. C. O. HAWTHORNE expressed his appreciation of the very thoughtful suggestions contained in the address. It was obvious that within recent years medical men had been passing into a new phase in connexion with asthma. Numerous questions, both pathological and clinical in their nature, had been raised. The hopeful feature about the suggestions which had been offered in the address seemed to him to be that they were proceeding now to deal with causes of symptoms which before were regarded as diseases and treated in an empirical fashion; in other words, a case of asthma had become, not an opportunity for a label and for a more or less traditional prescription, but an opportunity for a specific investigation of the individual patient, with a view to determining the causation in the patient and to treatment based upon causation.

Dr. A. LAPTHORN SMITH said that he was especially interested in the relation of eclampsia to asthma. He was quite sure that both eclampsia and asthma were due to poison in the blood. He asked whether people who were heavy meat eaters and who did not take much oxygen to burn the meat up were more liable to asthma than were vegetarians or fruit eaters. It had been his experience that they were. He had found remarkable benefit, not from putting more poisons like beliadonna and stramonium in, but from eliminating the poison which was present. The best drug he had met so far was potassium bicarbonate. He had rarely found that a patient did not get better if he persisted for several weeks in taking two or three drachms a day of potassium bicarbonate, with perhaps a little lemonade. He would like the members of the Section to try the alkaline treatment of the spasm of asthma, as he had tried very largely the alkaline treatment of the spasm of eclampsia.

The PRESIDENT in a brief reply expressed his thanks for the very kind reception given to his address. With regard to Dr. Coke's remarks about locality, he had been rather impressed by the case of a medical man (an asthmatic) who told him that, having had an attack of asthma in a particular place, and having occasion to sleep there a second time, he felt sure he was going to have an attack, but said to himself, "This is merely a question of your getting the idea that you must have an attack if you come to this place. There is no real basis for it, and you will not have an attack." And he did not. He had been to the place repeatedly since and had not had an attack. With regard to Dr. Lapthorn Smith's point about meat, he imagined that patients who were affected in the way described would be found by Dr. Coke's tests at any rate to be unduly sensitive to some form of meat protein.

ARTIFICIAL PNEUMOTHORAX.

A MEETING of the Cardiff Medical Society was held on October 10th, the chair being taken by Mr. W. MARTIN. Dr. PARRY MORGAN read a paper on "Artificial pneumothorax in the treatment of pulmonary tuberculosis." He pointed out the unanimity with which those who expressed their views on treatment in the report of the Medical Research Council's Committee praised this treatment, but noted that

there was not the same unanimity regarding what were suitable cases. Some advocated it only as a last resort—an extraordinary attitude towards an admittedly successful method of treatment. He gave reasons for the limited application of the method, and thought that the treatment should be commenced earlier than was usually the case. Complete collapse was not then necessary, for the gas in a partial collapse gave differential rest to the affected inelastic part of the lung, while the normal portions continued to expand freely as before. He compared artificial partial pneumothorax with the spontaneous collapse due to fluid or leakage from the lung and pointed out that, although the collapse was temporary in these cases, it was often followed by good results. He argued that temporary artificial collapse might be expected to be followed by good results, and quoted recent publications to show that this was the case. These also showed that the objection to producing an early artificial collapse, that it was nearly always followed by adhesions, did not hold in the case of partial collapse, as adhesions did not as a rule follow, nor did fluid appear, provided the intrapleural pressure was always kept negative. The treatment, therefore, was applicable to both sides at one and the same time.

Dr. Parry Morgan also described the two manometer apparatus for performing the operation, and insisted that its alleged complication was more apparent than real. There were only two differences between it and the single manometer apparatus, such as that of Drs. Lillingston and Vere Pearson: (1) Instead of a clip to shut off the gas there was a contrivance by means of which the gas could be cut off very gradually; when this was done, the pressure in the manometer gradually fell from that in the reservoir to that in the pleural cavity, instead of being either the one or the other. (2) A second manometer was introduced, which merely registered the reservoir pressure, whether the gas flowed or not. He showed that the physical conditions obtaining in the chest could be reproduced by using a rubber-stoppered bottle with the bottom cut out, filled with water, and inverted into a bowl of water. By means of this device he was able to demonstrate all the details of the operation, and to show clearly the advantages of the apparatus. From his demonstrations it appeared that no calculations were necessary.

Reviews.

MEDICINE IN THE WAR.

[SECOND NOTICE.]

LAST week (p. 691) we dealt with certain general considerations arising out of the first of two volumes on Diseases of the War, forming part of the *Medical History of the War*.[1] There remain to be dealt with the chapters dealing with the several diseases. There is a list of contributors at the beginning of the volume, but as many of the articles are joint compositions with the shares unapportioned it is sometimes impossible to ascertain which of the writers is responsible for particular statements.

The chapter on the enteric group of fevers contains interesting comparative records of other wars, which if they do nothing else indicate the supreme value of anti-typhoid inoculation. This point is further emphasized by the prevalence of paratyphoid A and B when the inoculation was against *B. typhosus* only and the corresponding decline when the triple inoculation was employed. In the discussion of the etiology very little new information is given; by no means can the presence of infection be avoided, but individuals can be rendered immune. Although this sums up the whole experience of the war, Wright, whose discovery made this immunity possible, is not mentioned either in the text or bibliography. The symptomatology and treatment is well summarized; there is little that is new, and there appear to have been no distinctive features in any of the theatres. Paratyphoid has received more particular study than ever before owing to the opportunities afforded by its occurrence apart from typhoid during the early part of the war. It is stated, correctly enough, that to distinguish the paratyphoids from one another is impossible, and from typhoid, as a rule, difficult. The importance of recognizing the cases of paratyphoid that do not resemble typhoid and the difficulty of doing so are not made so clear.

The dysenteries are admirably dealt with, and this chapter contains much that is new and most necessary to be widely known, particularly as to the sequelae that cause invalidism. The measures found imperative for the after-treatment of convalescents are well and adequately described. Stress is rightly laid on the need for evacuating all cases of diarrhoea to field ambulances at once, as well as on the precautions to be taken with "suspected" dysentery. The dangers of waiting for bacteriological confirmation of the diagnosis are not exaggerated.

The story of cholera is well told. If the figures given are exhaustive it would seem that 4,436 cases of cholera occurred in the Mesopotamian Force, as against 3,117 of the enteric group. The case mortality was appalling; it amounted to over 40 per cent. The protection afforded by inoculation with cholera vaccine is unfortunately short-lasting, and inoculated persons who contract cholera show nearly as high a mortality as the uninoculated. The control of the epidemic must have been due quite as much to sanitary precautions as to inoculation. A high proportion of cases thought to be severe diarrhoea only were found to harbour cholera vibrios. Presumably, although this is not stated, the early segregation of cases of diarrhoea was insisted upon, and with somewhat the same degree of success as attended this measure when applied to dysentery. Typhus, as we remarked in our first notice, caused little wastage in the British army.

The research conducted into the etiology and transmission of cerebro-spinal fever is perhaps the most successful and extended medical investigation of the war. For this the navy especially deserves our thanks and congratulation. The conditions were perhaps simpler, seeing that the bulk of cases occurred in England, where access to the best laboratories in the country was available. Still the fact remains that our knowledge of this disease was advanced during and by the war more than that of any other disease. To Glover belongs the credit of making one of those great observations that are magnificently simple: "The space between beds determines the 'carrier' rate; it is carriers, not cases, that spread the disease. Dépôts with beds too close together will have epidemics of cerebro-spinal fever; if the beds are far enough apart the disease will cease to occur." We learnt, moreover, during the war the immense curative value of antimeningococcal serum administered intraspinally and *persistently*.

Influenza yields a chapter that is a record of a visitation of the angel of death which, had it come upon the world in peace time, would have overwhelmed us all with consternation. As an incident in the world war it merely added to its horrors. Here again the record is of research patiently and hopefully carried out under stupendous difficulties, to end only in disappointment. At the end we seemed to have added only one fact to our knowledge of the etiology—namely, that the disease was caused by a filter-passing virus. No curative remedy was discovered, no preventive measures proved successful. The pandemic appeared to cease by killing the most susceptible individuals and by immunizing the more resistant after they had survived one or more attacks. Scarcely anyone escaped entirely. The bare recital of the numbers affected fails utterly to give a picture of the state of our hospitals in France during the height of the epidemic. It may be as well that there was no Defoe amongst us to hand down to posterity the dreadfulness of those scenes of mud, darkness, and misery.

The chapters in which malaria is described are patterns of military medical history. The account given of the geography of the disease, the natural history of the mosquito, and the effects of malaria in the several theatres of war constitute a veritable masterpiece whether regarded from the standpoint of medicine, military annals, or literature. The clinical account of the disease has surely never been surpassed for accuracy or graphic detail; the discussion on treatment in both the acute and chronic stages will stand for many years as the authoritative guide. On page 285 is a short account of the handling of the malaria-infected battalions sent to France as reinforcements. Never were men more sorely needed in the front line, never perhaps did a military commander exhibit greater restraint than on this occasion when medical advice was allowed to prevail. Whole battalions were sent sick rather than individuals, with the result that in two months the ague-stricken men took their place in the hardest fighting

[1] *The Medical History of the War Based on Official Documents: Diseases of the War.* Vol. i. Edited by Sir W. G. Macpherson, K.C.M.G., with the assistance of Sir W. P. Herringham, K.C.M.G., Colonel T. R. Elliott, C.B.E., D.S.O., F.R.S., and Lieut.-Colonel Andrew Balfour, C.B., C.M.G. London: H.M. Stationery Office, or through any bookseller. 1922. (Medium 8vo, pp. viii + 550; 6 coloured plates and other illustrations. £1 1s. net: post free £1 2s.)

with less than average wastage from sickness of all kinds. Here is a lesson not to be overlooked by combatant officers, to whom indeed the credit of having made the experiment possible is chiefly due. Trypanosomiasis and the relapsing fevers did not loom large in their influence on military operations, but our world-wide empire may some day find the facts recorded of great importance.

Phlebotomus or sand-fly fever proved especially troublesome in the Near East and in Mesopotamia. The treatment was preventive, and required ruthless efficiency in fly destruction.

In a short article a very good account is given of trench fever. This was a most mysterious disease. Evidently it was spread by lice, but how is a question still unanswered. American opinion believes that it can be transferred from man to man by the simple bite of the louse; British observers think that the entrance of the faeces of the infected insect into a bite, scratch, or abrasion is essential. This difference of opinion detracts in no way from the credit attaching to the happy observation of McNee, Renshaw, and Brunt that the virus was blood-borne. The invalidism caused by trench fever was probably higher than that caused by any other disease of the war. It is singular that although relapses appear to be frequent and the symptoms are protracted the disease was not introduced into England either during or after the war. Yet the infected men and the lice certainly arrived in this country. We are not convinced that the explanation offered for this phenomenon is correct. There seemed to be districts even in France where trench fever did not flourish, despite the lice. Spirochaetal jaundice was an interesting disease seen occasionally; it attracted attention because of the dramatic course run by fatal cases and because of the comparative novelty of spirochaetal infections in general. As a war disease it counted for little. In the East epidemic and catarrhal forms of jaundice were fairly common; more than one organism was probably responsible.

Of all the diseases prevalent during the war none, perhaps, were subjected to more thorough scientific investigation than the group known as the food-deficiency diseases. Before the war we knew of their existence; it had also been discovered that foodstuffs contained certain accessory nutritional factors or vitamins. During the war the necessities of whole nations trying to live on the scantiest food supplies stimulated even governments to study food problems scientifically. Disease among the troops and famine among the civilians provided only too much experimental material for observation.

Thus it came about that our knowledge of four diseases—scurvy, beri-beri, famine dropsy, and pellagra—was advanced during the war beyond all expectation. Lind's book on scurvy was published in 1754; the next book in English was written by Hess in 1919, and practically the whole of the new work it contained had been done during the war. It was not until the war that Miss Henderson Smith discovered that lime juice was relatively useless. Ration lime juice failed to cure scurvy in Mesopotamia. The facts will bear retelling at length. "Lime juice" was introduced as a ration for the British navy in 1804, but down to 1875 lemon juice was in fact issued under this name. Since 1875 the "lime juice" supplied has been true lime juice from the West Indies, and would therefore be much inferior in antiscorbutic value to lemon juice. On this point control experiments were made in Northern Russia in 1919 by Captain A. J. Stevenson, who found that the addition of four ounces of lemon juice freshly prepared from fresh lemons gave markedly beneficial results. The Report of the Royal Society War (Food) Committee is clear and definite: "West Indian lime juice, as ordinarily prepared, is useless for the prevention of scurvy. Fresh limes have an antiscorbutic action, but their efficiency is only one-fourth that of lemons. . . . Nares's expedition of 1875, notorious for the serious outbreaks of scurvy encountered, was the first to be provisioned with 'lime juice' prepared from West Indian limes." If as seems to be the case, judging from an answer to a question in Parliament, the navy is now using West Indian lime juice again, scurvy will reappear should the remainder of the ration at any time fail in adequate antiscorbutic vitamin, unless, of course, lime juice is supplied merely as a pleasant summer drink and some special antiscorbutic ration is being issued. The great outbreak of scurvy among Indian troops in Mesopotamia produced a state of affairs that can only be likened to what happened during the Walcheren expedition. The temperate tone of the article is surprising considering the circumstances. The writer is to be commended for concentrating attention on the admirable way in which remedies were found rather than on the causes which gave rise to the disease. The discovery of the antiscorbutic properties of germinated lentils and peas together with that of the value of "marmite" as a preventive of beri-beri were two remarkable achievements. In these two preparations a portable ration has been obtained which should safeguard our troops in any future campaigns. Any doubt, too, that beri-beri is a food-deficiency disease has now been dispelled by the researches and experiences of the war.

Famine dropsy was a disease of civilian populations and prisoners of war rather than of armies. Nevertheless it was so purely a war effect that it is properly included in this history. The most interesting part of this chapter is the bringing to light of the excellent but long-forgotten work of Dr. Cornish in the Madras famine of 1877–78. Seeing that the reward of his services at the time was the removal of his name by the Calcutta Government from the list of honourable recommendations, the tardy justice done to his memory is welcome. Pellagra is a disease the precise etiology of which presents a problem as yet unsolved. Wilson's observations, however, on the biological value of protein are of the utmost importance to the scientific study of dietetics. His conclusion cannot be gainsaid: "Lack of sufficient biological value of protein stands in etiological relation to pellagra, certainly as an exciting factor and possibly as the determining factor." The report of the Committee of Enquiry regarding the prevalence of pellagra among Turkish prisoners of war will stand for years to come as a record of one of the best and most scientific inquiries ever accomplished by a nation at war.

The frequent occurrence of nephritis among the troops in France and Flanders gave rise to the impression that there might be some peculiar form of nephritis to which troops in a war area, and especially those serving in the trenches, were liable. The survey of the malady and of the investigations into its nature make it probable that it is not a distinct and separate form of nephritis. The unusual frequency of dyspnoea, dropsy, and uraemia in soldiers is perhaps to be explained on the ground that the exertions demanded of soldiers go to the utmost limit their strength permits. The prognosis from the point of view of ultimate invalidism and pension claims is a matter of vital importance; it is to be regretted that so little information has been collected on this aspect of nephritis attributable to war service.

Cardiovascular disorders formed a large group of medical casualties and since the war have presented pension problems of special difficulty. It seems possible that the importance of minor degrees of organic valvular disease was underestimated whilst the war was in progress. This chapter alludes to the number of men seen by pensions boards whose subsequent malady has assumed the form of progressive infective endocarditis. On this point the writer might have spoken with greater emphasis and certainty. The evidence is weighty and still accumulating. Medical opinion should be expressed far more firmly on this matter, as it is one of the principal points on which the recommendations of medical boards are sometimes not followed by the Ministry of Pensions. The discussion of functional disorders of the heart is one of the best that has been written on a most troublesome problem. The short description of treatment and prognosis contains in a readable epitome a great deal of information gathered by many painstaking observers and patient commandants of convalescent camps. In conclusion, it may be said that if the other volumes of the *Medical History of the War* are as good as this one medicine as a whole will have gained much advantage from its publication.

COMPARATIVE ANATOMY.

A Laboratory Manual for Comparative Vertebrate Anatomy,[2] by Professor HYMAN of Chicago, constitutes a successful effort to provide a practical book for the study of zoology arranged on the comparative plan. In the preface the author indicates the advantages of the comparative over the type plan, and the manual fully justifies the claim that by the former method a student acquires an understanding of vertebrate and human structure which he is unable to attain by the latter. The book may be highly recommended to medical students, since it gives such a clear conception of the "history of the human body," and is not overburdened with details which obscure fundamental principles.

[2] *A Laboratory Manual for Comparative Vertebrate Anatomy*. By L. H. Hyman. Chicago: The University of Chicago Press. 1922. (Double roy. 16mo, pp. xv+380; 69 figs. 2.50 dollars.)

The manual is essentially a guide to practical work, but a necessary amount of explanatory matter is included, and an important feature is the insertion throughout of references to standard textbooks which may be consulted most profitably for the acquisition of further and more detailed knowledge upon particular points. A study of the book makes it manifest at once that the author is not only possessed of wide knowledge, but is also a teacher who has found the best method of presenting his subject. The descriptions of the structure of the various animals are given in a clear and simple style, and care has been taken to define as accurately as possible the meaning of all technical terms. The animals described have been selected with great discretion, and with the chief object of presenting as directly as possible the story of the evolution of the various systems. A helpful feature is the insertion at the end of each chapter of a good summary, which briefly correlates the most important facts before the reader passes on to new ground. For a first edition the manual is remarkably free from mistakes, although on page 311 there is a very obvious one, since we are informed that the abducens nerve "penetrates the *eyeball* at the point of origin of the external rectus muscle."

The manual is not so liberally illustrated as most of its kind; there are only sixty-nine illustrations, but these are simple, clear, and sufficient, since the instructions for dissection are so arranged that any student ought to be able to display and examine all the features referred to in the text and make his own diagrams direct from his dissections. The book concludes with a good index and two appendices, the first dealing with the pronunciation and derivation of technical words, and the second with methods of preparation of materials.

TESTICULAR GRAFTS.

WE have received from Dr. SERGE VORONOFF, director of the laboratory of experimental surgery of the Collège de France, Paris, a copy of his illustrated monograph,[3] in French, on the subject of testicular grafts, and also an English translation of a paper on the subject, which he had intended to read to the thirty-first French Congress of Surgery in Paris this month; some objection, however, was, we understand, taken, and the paper was, in fact, not read. Dr. Voronoff afterwards gave a lecture which seems to have attracted a good deal of attention in the general press; it may, therefore, be convenient that we should give an early account of Dr. Voronoff's claims.

Dr. Voronoff reported the results of 120 testicular grafts performed on animals to the twenty-eighth French Congress of Medicine in October, 1919, and since then he has applied to man the method which he had practised on animals. The first experiments were made by grafting the testicle of a lamb upon a female sheep; the apparent result was that the growth of bone of the limbs of the progeny of the sheep was modified, the legs being shortened. In other experiments testicles were grafted into young female goats from a male; the ovaries had previously been removed; the goats remained small, their limbs were shorter, and their horns were thicker than those of normal goats. Numerous testicular grafts were made upon castrated animals and he-goats in order to ascertain if transposed testicles could play their ordinary part in animals deprived of their own testicles, and replace them in their functions as endocrine glands. In one case, which Dr. Voronoff gives in detail, the grafted testicles apparently performed their functions as endocrine glands, dominating the secondary sexual characters of the male animal, his sexual instinct, and his virile capacity. Other similar grafts made upon castrated he-goats and rams gave analogous results in every case in which the graft did not slough or was not too quickly absorbed. Testicular grafts were also made upon two senile rams aged about 10 years, with the result that two months later a change in their bearing was observed, and their broken-down air and dull look gave place, it is stated, to a vivacity of movement and a bellicose disposition. Their vigour increased considerably from month to month, and, isolated in separate pens with female sheep, one became the father of a lamb and the other of two. In one of these rams the grafted testicular fragments were removed at the end of fourteen months, when the condition of the animal declined once more and its vivacity disappeared; a new graft was then made, and—while not so rapidly as on the first occasion—some mon hs later there remained no trace of its senile condition. Several photographs of these animals are given. This experiment, which was repeated several times upon other old animals, apparently showed that the graft could be renewed and successive rejuvenations obtained.

Dr. Voronoff gives particulars of a number of cases of testicular grafts from cynocephalus monkeys which were made upon men between June, 1920, and February, 1921.

In the first two cases the patients had suffered from tuberculosis of the testicles, which had consequently been removed; the grafts provoked latent suppuration and proved unsuccessful, although in the second case the man's beard had commenced to grow again.

The third case was that of a man aged 59, who had had gonorrhoea as a youth, and complained of loss of memory, decreased capacity for intellectual work, and physical depression. Testicular grafting resulted in much improvement in his general health, and he is stated now to experience a physical well-being long unknown to him. He has recovered his memory and his mental vigour has been renewed.

The fourth case was that of a celebrated man of letters, aged 61, who, before the graft, was of a senile type, with wrinkled face and dull eyes; his mental activity had become slow and his memory had failed. Two years after a testicular graft had been performed he recovered a virility he had lost for ten years, was well set up, and gave an impression of youth and vigour; he took long walks and could again work for long hours without fatigue.

In the fifth case the subject was an American, aged 33, who was neurasthenic, melancholic, and dyspeptic. A year after the testicular grafting he enjoyed perfect health and virility, and although Dr. Voronoff suggested to him that a romantic love affair which had ensued might have played a more important part than the graft the patient repudiated the suggestion.

The sixth case was that of a South American of 66, who suffered from senility and depression, aggravated by gastric ulcer; after a testicular graft had been performed he experienced physical well-being and a new sexual vigour.

An Englishman of 74, whom Dr. Voronoff had arranged to exhibit to the Congress, was the seventh patient. Two years before the graft this patient seemed to be an old man, inert, obese, haggard, walking painfully with the aid of a stick. Eight months after the graft was performed he appeared before Dr. Voronoff with half his *embonpoint* gone, his aspect jovial, and his movements active. His condition was stated to-day to be still that of a man who had recovered twenty to twenty-five years of his lost youth. Several photographs of this patient, in vigorous attitudes, are given in the monograph.

Dr. Voronoff does not admit the possibility of grafting the testicles of animals other than monkeys upon man, and human grafts are apparently impossible to procure. From February, 1921, to March, 1922, he was unable to obtain any large monkeys, and no further grafts were made until March, 1922; the results of these later cases were too recent to afford proof of benefit.

Dr. Voronoff thinks that he has established the efficacy of testicular grafting in senile conditions due to the insufficiency of the internal testicular secretion, and not due to affections causing deterioration of essential organs of the body. He does not attribute senility only to the diminution or disappearance of the internal secretion of the genital glands, but he thinks that human force and energy are bound up with the activity of the sexual glands. It is logical, therefore, in Dr. Voronoff's view, to suppose that by supplying the testicular hormone to the body at the moment when its own glands no longer elaborate it the body is really rejuvenated. Whether the testicular graft would be of utility under other conditions, as an organic stimulant of the enfeebled functions of other endocrine glands or of tissues upon which the testicular hormone had a more direct action, are, he states, questions to be decided by further experiment. Placed under the skin or under the muscles, the grafts, even under the most favourable conditions, only survive in part: the centre necroses; the cortex usually survives but is reabsorbed in a few months. Placed, however, in such a position as the peritoneal cavity or the tunica vaginalis, where exudation can be made rapidly, the graft may survive for several years. A graft of the entire testicle may be made, but it must be of small volume and young; for the most part the grafting of fragments is preferable. Dr. Voronoff advises that the testicle should be divided into two, four, six, or eight portions according to its size, the tunica albuginea scarified lightly, and the fragments attached by a catgut stitch above and below in such a way that the glandular face is in contact with the tunica; the fragments should not touch each other, but should be everywhere in direct contact with the tissues of their new host. Dr. Voronoff now supports the view of Professor Retterer, who affirms that the epithelial cells are the agents of the cellular secretion, both internal and external, and that the seminal cell is at once exocrine and endocrine. When in testicular grafts these cells are deprived of the excretory canal they cease to elaborate the spermatozoa, and act only in the endocrine sense; to the interstitial cells a nutritive function is attributed.

[3] *Greffes Testiculaires.* By Dr. S. Voronoff. Paris: G. Doin. 1922. (Roy. 8vo, pp. 83; 19 illustrations.)

CONTROL OF CONCEPTION.

LADY BARRETT has written an essay of less than fifty pages on *Conception Control*,[4] in response to requests for some statement regarding the individual and national aspects of this widespread practice. She writes as a medical practitioner and a specialist in gynaecology; but her object is not so much to give medical advice as to place before the lay reader her considered opinion upon the social and hygienic issues underlying the problem. Her view is that medical advice on this matter is best given to the individual by his or her medical adviser, and must depend on the circumstances; whether control of conception shall be practised when husband and wife are healthy is, she holds, an ethical question for the individual pair, and not a medical question to be decided for the public at large. She is concerned at the careless advocacy of an indiscriminate use of contraceptives, believing that if this goes on various harmful effects will soon show themselves: "In the whole discussion of this subject it is important to keep in mind that the physical is only one aspect of the sex relation."

So far as we understand her, Lady Barrett takes a broad view of present-day difficulties. For ordinary couples she seems to favour some measure of voluntary self-control, not merely for its spiritual and aesthetic advantages but as a practical means of limiting the size of a family, while living a natural married life. The method she finds least harmful for controlling conception is "the avoidance of those parts of the monthly cycle in which conception most commonly takes place" (p. 35), but she does not add that the efficacy of confining intercourse to one week in the month has been challenged. She detects serious fallacies in the arguments of those who advocate the widespread teaching of mechanical and chemical methods of preventing conception. In particular she deprecates the current doctrine that child-bearing may safely be postponed by young married women, and that children will come in abundance to all unless actively prevented—"Advice which may be quite necessary in cases of ill health or special conditions may be fundamentally wrong to give broadcast to all." It is her experience that, as a result of the ideas conveyed to the minds of young people by books and speeches, many of them though anxious to have children "start married life with sordid and unnatural preparations for a natural act" in accordance with the vogue of the moment. Her own belief is that, apart from those who for physical reasons or because of hereditary taint should not bear children, married love ought to express itself at once in the usual way without the use of artificial contraceptives. Notwithstanding the efforts now being made in "birth control clinics" for working-class women, she is convinced that this propaganda will have a disastrous effect on the differential birth rate of the community, "for the birth rate amongst the least intelligent, least efficient, and the mentally deficient, will be unaffected . . . the diminishing fertility of the more capable classes is a national peril."

Lady Barrett's position seems quite clear: wherever child-bearing is fraught with danger conception must be prevented, by means chosen to suit each special case; but public instruction in the use of contraceptive devices is generally misapplied and is therefore detrimental both to health and morals. She does not agree with the "spacing" of families by the regular use of artificial checks; if the family is to be limited to three or four it is better for everyone concerned that the children should all be in the nursery together, and later all at school. Her objection on medical and aesthetic grounds to most of the usual contraceptives is stated very plainly in Chapter III; though here and elsewhere throughout the book her language is commendably restrained, and all personalities are eschewed.

What solution does Lady Barrett suggest for the large problem presented by the falling birth rate among the educated and prosperous, and the excessive childbearing among the poorer and less efficient classes? She accepts the view that fertility, broadly speaking, varies inversely as the nervous energy or general vitality; and her remedy is to raise the standard of living among working-class mothers and to advise a simpler life for the more richly endowed.

We have tried to give an uncoloured account of the purpose and manner of this able and thought-provoking tract, which should be read by all medical men and women.

[4] *Conception Control and its Effects on the Individual and the Nation.* By Lady Barrett, C.B.E., M.D., M.S., B.Sc., Consulting Obstetric and Gynaecological Surgeon to the Royal Free Hospital President of the Federation of Medical Women. With a foreword by the Archbishop of Canterbury. London: John Murray. 1922. (Cr. 8vo, pp. 48. 2s. net.)

THE DISCOVERY OF THE CIRCULATION.

THE first volume of the series of Classics of Scientific Method, which is designed for the instruction of those without any previous knowledge of the subject, is on *The Discovery of the Circulation of the Blood*,[5] and has wisely been entrusted to Dr. CHARLES SINGER. The description of the circulation is therefore rightly elementary, but most medical men will find that they can learn a good deal from the clear historical survey of the records and workers by whom the full discovery of the circulation was achieved; the biographical details add to the interest of the well-told story. After a sketch of the knowledge of the vascular system in antiquity, in which reference is made to Hippocrates, Aristotle, and more fully to the views of Galen, Dr. Singer passes on to the period of the Renascence and to Leonardo da Vinci (1452–1518), who, he holds, was the first to question the authority of Galen and to give a more correct description of the anatomy of the heart; he intended to publish a textbook of anatomy and physiology, but did not live to do so, and accordingly the epoch-making appearance of the first great book on anatomy and physiology since Galen's time was the *Fabrica* of Vesalius in 1543, in which the statements of Aristotle and Galen were tested by the author's own observations; doubt was thus cast on the received belief that the blood passed from the right to the left ventricle through the interventricular septum of the heart. A little later Michael Servetus, a contemporary of Vesalius and the martyr victim of Calvin, made known the circulation through the lungs in a book which was burnt with its author at the stake in 1553. Of Vesalius's successors, Fabricius of Aquapendente was professor for fifty-four years at Padua, and thus was the instructor of William Harvey, who in his Lumleian lectures at the Royal College of Physicians of London set forth his great discovery in 1616, and gave it a wider public by publishing the *De Motu Cordis* in seventy-two small pages at Frankfort in 1628. The discovery of the capillaries and red blood corpuscles is illustrated by reproductions of plates from Malpighi and Anthony van Leeuwenhoek's works. This is a book full of information presented in a pleasant style, with good illustrations, and at a remarkably cheap price.

MERCHANT SEAMEN.

IN a review published under this title last week (p. 692) reference was made to a paper on the preservation of the health of seamen on board ship, read by Dr. Arnold Chaplin to the Section of State Medicine of the Royal Society of Medicine. In the endeavour to be brief a phrase was used which did not adequately express Dr. Chaplin's opinion, and may have conveyed the impression that in his view all shipowners were negligent and careless concerning hygiene. This is far from the case; otherwise the big lines would not have appointed competent medical advisers and able medical officers. We may perhaps best put the matter right by quoting the paragraph from Dr. Chaplin's paper:

"A ship, as we understand it to-day, is the result of innumerable compromises, and this fact can hardly be insisted upon too strongly. The passenger would prefer the ship to be all luxury and comfort, the sailor easy to handle, and the owner economical to work. So also where health is concerned; the hygienic enthusiast would build his ship much as he would a sanatorium or a hospital, and he might regard the owner as unreasonable if he pointed out that his ship was designed, not to carry sick people, but passengers and cargo. In every department of a ship, whether it be the engine room, the crew quarters, the cargo space, the passenger quarters, or the medical arrangements, the final result has been obtained by means of compromise. This means that no department on board ship can approach the ideal without encroaching upon some other department. I would therefore beg of you to dismiss from your minds the idea that the principles of hygiene on board a ship can be satisfied in the same way as on shore, where space has few limitations. I would also ask you to remember that the main object of a ship is to carry on our commerce, and that nothing should be done to interfere with that object so long as the reasonable laws of health are obeyed."

Dr. Arnold Chaplin's paper, "Certain measures for preserving the health of seamen on board ship," was published in full in the *Proceedings of the Royal Society of Medicine*, 1921, vol. xiv (Section of Epidemiology and State Medicine), pp. 23–37.

[5] *The Discovery of the Circulation of the Blood.* By Charles Singer, M.D., D.Litt., F.R.C.P. Classics of Scientific Method, edited by E. R. Thomas, M.A., M.Sc. London: G. Bell and Sons, Ltd. 1922. (Cr. 8vo, pp. x + 80; 8 plates. Price 1s. 6d.)

British Medical Journal.

SATURDAY, OCTOBER 21ST, 1922.

THE WHOLE ART OF MIDWIFERY.

THE sketch of the development of the Edinburgh school of obstetrics and gynaecology with which Professor Watson began his inaugural lecture, published at page 712, is full not only of historical interest but of instruction useful to us to-day.

To Edinburgh belongs the honour of being the first teaching centre to have a professor of midwifery; nearly two centuries have elapsed since the town council appointed Joseph Gibson, a surgeon, "to be professor of midwifery in this city." His duty was to teach women midwives, and he and his successors discharged it so well that by the beginning of the nineteenth century it could be said that there was "scarcely a parish of Scotland the midwife of which has not been regularly taught." Though the town council of Edinburgh was the patron of the university, the relations of the one to the other were not always smooth. The council was proud of the university, but occasions for strife arose, and the university did not at once recognize the new chair. A century was indeed to pass before it made midwifery a compulsory subject in the medical curriculum. Ten years later, in 1840, the university was fortunate enough to secure for the chair of midwifery a man of genius, and James Young Simpson's strong personality, originality of mind, and inspiring teaching firmly established obstetrics and gynaecology as an essential part of the medical curriculum. He and his successor, Alexander Russell Simpson —and between them they cover sixty-five years of the Edinburgh school—were both obstetricians and gynaecologists, and this brings us to the kernel of Professor Watson's address. The two subjects, especially when we regard prevention of disease, are one, and that their recent divorce at many schools in America and Europe was a retrograde step is the opinion in Edinburgh and Toronto, two of the wisest and most progressive medical faculties among English-speaking peoples. It is a curious example of the swinging of the pendulum and of the solidarity of the profession in Great Britain and the great Dominion that when in 1912 Toronto was looking for a professor to unite the two chairs it found the man it wanted in Edinburgh, where until a few years earlier the two had been one, and that when Edinburgh determined to abandon the experiment of division it had begun seventeen years ago it brought back its old alumnus to occupy the single chair it had restored. In Edinburgh the evils of division were hidden because Sir Halliday Croom, appointed to the restricted chair of obstetrics, was not less eminent in gynaecology, and Dr. Freeland Barbour, appointed lecturer in gynaecology, had won his spurs in obstetrics.

When we examine the reasons Professor Watson gives for his very decided opinion against separation we find the chief to be bound up with the pioneering work of another ornament of the Edinburgh school, Dr. J. W. Ballantyne. For a long time he got very little encouragement, but he was given courage to persevere by the consciousness that he was building on a sound basis of pathological observation. It is a little over thirty years since he published his *Introduction to the Diseases of Infancy*, a book many laid down with the observation that the author had never got to his subject, for much of it was about the anatomy of the newborn infant, a little of it about pathology, and very little about clinical appearances. This criticism was mistaken, as experience, reflection, and the persistent and persuasive advocacy of the author have proved; so true is this that, like some other pioneers, Ballantyne is at risk of his early work being forgotten owing to his very success in making his doctrine the common possession not only of the profession but of the public.

We may leave this part of the field, where the battle has been won, for another, where, though the final result is not in doubt, it has not yet been attained. Preventive midwifery is not quite a new subject, but its rational study on scientific lines is. Professor Watson gives a definition of preventive midwifery: it is ante-natal care during pregnancy, a minimum of interference during labour, the most rigid attention to asepsis, and the earliest possible carrying out of surgical intervention when it is required after the puerperium. Here again Ballantyne was a pioneer, and even those who have followed the subject most closely will be surprised by the reminder that it is over twenty years since he made his first plea for a pre-maternity hospital.[1] Professor Watson, who speaks from nearly ten years' experience in the obstetrical department of the Toronto General Hospital, says that a pre-maternity or ante-natal department is the most essential part of a maternity hospital.

Professor Watson only accepts the midwife with reluctance; her training, he says, interferes with that of the medical student, and her want of general medical education limits her usefulness in the all-important work of ante-natal care. The second objection is well grounded and might perhaps be extended to post-natal care also. The promoters of the Midwives Act were mainly influenced by the fact that, in England at least, attendance in a large proportion of cases was given by women who, while they called themselves midwives, had not learnt the essential importance of cleanliness, had not been taught to recognize conditions arising during labour which made skilled medical assistance necessary, and were unable to appreciate when surgical aid was afterwards needed. The Midwives Act has not done all its promoters hoped; there are still many untrained women and some men acting as midwives, and some of the untrained women certified when the Act came into force are still in practice. But on the whole it has done good, and any scheme for the improvement of the actual practice of midwifery must take the certified midwife into account, though no effort should be spared to increase her sense of responsibility, and in this we would include full realization of her own limitations.

This is not to say that the medical profession is to lose control of midwifery. If the ideal put before us is to be realized, and when it has been realized, there will be much highly responsible work for the general practitioner to do. The first task is to convince women of the value of consulting their doctor at an early stage and at intervals afterwards to make sure that all is right. They already do so to a considerable extent, and there will probably be no great difficulty in inducing them to make it an established custom. Professor Watson says he has found none. There may be a little more difficulty in persuading them to come again a month after delivery. The troublous time is over and they have new interests; but the request is so rational that it will appeal to the reasoning faculty in women, and, as we all know, they can be very rational on occasion. All this implies the establishment and maintenance of a high standard in the

[1] It was published in our columns on April 6th, 1901 (p. 813).

teaching of obstetrics and gynaecology to medical students, a subject on which Professor Watson touched rather lightly at the end of his thoughtful and stimulating address.

Thus we come back to what we have ventured to call its kernel—the essential unity of obstetrics and gynaecology, and the imperative need to teach them as one subject. Thus also we justify the title of this article, for the whole art of midwifery does not consist in procuring the safe delivery of a living child, but in ensuring also that the mother shall not experience any preventable injury or later suffer from any remediable damage due to her having passed through the physiological process of childbirth.

MEDICINE AND THE RENASCENCE.

THE annual commemoration of its benefactors on St. Luke's Day, October 18th, is an occasion for the Royal College of Physicians of London to unbend. Harvey, when he founded the annual oration which bears his name, clearly stated his aims. His main objects were that the Fellows should be reminded of their chief duties as physicians—to be diligent "to search and study out the secrets of Nature by way of experiment; and also for the honour of the profession to continue in mutual love and affection." Harvey was not only the first of physiologists, but for the best years of his life actively engaged in practice—he was physician to St. Bartholomew's Hospital as well as to the King. He was therefore well qualified to admonish physicians, and his injunctions were so well and widely conceived that they succinctly express the aims of the British Medical Association, which seeks to apply them to the whole profession. The appeal of the orator extends far beyond the audience he actually addresses; Harvey desired the oration to be public, and the occasion affords the College an opportunity of inviting men of light and leading in other callings to hear the oration, and to share in the less formal celebration at the banquet which Harvey directed should afterwards be held.

Over two hundred Harveian Orations have been delivered, and if the injunctions of the founder had been less widely drawn, or if they had not been interpreted rather in the spirit than the letter by succeeding orators, the profession might have been treated to as many examples of formalism. But though every orator has felt it becoming to bring what he has had to say into some relation with Harvey, he was so many-sided a man, so endowed with intellectual courage and independence, so possessed with divine curiosity to search and study out the secrets of Nature, and so well versed in the learning of his time, that to find a new point of view from which to regard his work or illustrate it has not been a task as difficult as might have been anticipated.

The chief aim of the orator this year was, as may be seen from the full text of his oration published at page 707, to draw a picture of the kind of intellectual world into which Harvey was born. Just recently there has been a great pother among the log-rollers who write columns of "literary gossip" about the discovery of some of their number that Shakespeare was the child of his age, influenced by the currents of thought then prevailing and by the events of the day. For biologists the question is, not to affirm the influence of environment, but to determine where to trace its limits. Dr. Arnold Chaplin is too much of a man of science to consider it worth while to discuss this aspect of his subject, and too good a humanist not to assign great importance to the revival of learning in Western Europe which had begun about a century before Harvey's birth. The orator thus became involved in an attempt to explain the renascence. For him it dates fairly definitely from 1453, when the Ottoman captured Constantinople. He does not, however, altogether forget that the West had earlier been in contact with the Greek Empire. There had been the Crusades; no doubt the motives of the men who were swept off their feet by that remarkable movement were mixed—in part they were religious and a reaction against Mohammedan propaganda and conquest, in part they were due to a love of adventure, but in part also to a new spirit of inquiry and curiosity. The same age saw the birth of scholasticism, founded on the study of the masters of Greek metaphysics, of whom Aristotle, the chief, enjoyed a kind of mystical authority, so that the conclusions even of his commentators could not safely be questioned. The more settled conditions of government which Western Europe owed to the administrative genius of the Normans not only made the Crusades possible, but favoured the growth of learning within the cloister, and also, though to a very limited extent, outside it. There was already a certain stirring of men's minds in Western Europe; Dr. Chaplin probably comes very near the secret when he tells how the Church became aware that its power needed support to be obtained only from an appeal to the intellect. As evidences of this we may note in passing that the University of Bologna came into existence in the middle of the eleventh century and had acquired a European reputation as a school of jurisprudence before its end, and that the medical school of Salerno dates from the middle of the twelfth century. Western Europe had indeed been prepared to benefit by the opportunity given to it when Greek scholars fled from conquered Constantinople to find a refuge in Italy, and when the Ottoman readily sold original Greek manuscripts to the princes and merchants of that country. Hitherto the West had seen the ideas of the men of the great age in Greece through the dark glass of Latin translations or paraphrases of Arabian versions, and the same was true of the later Galen, whose writings, in spite of his own and his commentators' errors, had attained an authority as of holy writ.

Dr. Chaplin has done good service by tracing out what followed. The first stage was to establish the authentic Greek texts, to understand them truly, and to learn the lessons they had to teach. This was the task of the humanists, the task in medicine of the scholar-physician, of whom Linacre was one of the earliest—the earliest in England—and Caius one of the greatest. What was to be learnt from the great Greeks, however, was method rather than fact. Men began to perceive that it was unsafe to rely on written authority, and necessary to search and study out the secrets of Nature by direct observation and experiment. There was a change of spirit, and the first-fruits of the new spirit was the foundation of scientific anatomy by Vesalius; his teaching attracted inquiring minds from all over Europe, as Dr. Herbert Spencer more fully showed in the Harveian Oration of last year. Vesalius's great book was published in 1543, and three-quarters of a century later Harvey laid the sure foundation of the scientific study of function by his work on the heart. His little book swept away an enormous superstructure piled up on a basis of inaccurate observation and buttressed by fanciful conjecture. The impetus Harvey gave to physiological inquiry was, as the orator truly said, marvellous, and it is not yet exhausted. Rather it seems to gather strength; the growth of physiology and the immense influence it is exerting on the practice of medicine would have been a source of the deepest satisfaction to those great humanist scholar-physicians of whom Dr. Chaplin draws so pleasing a picture.

A CASUALTY DEPARTMENT FORTY-FIVE YEARS AGO.

THE Poet Laureate celebrates his 78th birthday on Monday next, and it is forty years since he retired from medical practice and gave up the posts of assistant physician at Great Ormond Street and physician to the Great Northern Hospital. His strictly medical writings have been few; the only one we can trace—if we except his recent helpful disquisition on the language of anatomy to which we referred at length on August 12th (p. 276)—is a note contributed many years ago on a case of severe rheumatic fever treated with splints. But in 1878 Dr. Robert Bridges wrote a remarkable paper entitled "An account of the casualty department,"[1] describing the medical work of the old surgery at St. Bartholomew's, where he acted as "filtrator" of out-patients from 9 o'clock till past noon, spending (as he found) on an average a minute and a quarter over each case. Some 158,000 patients were relieved at St. Bartholomew's Hospital in the year 1877. Pondering over this vast number he asked himself several questions, such as, "Where in the world do all these patients come from? What is the matter with them? Have they anything the matter with them? What becomes of them afterwards? Do they ever go there again? How is it possible to attend to them when they do go? How are they counted, sorted, recognized, prescribed for, dispensed for? What quantity and what sort of medicine do they consume? What does it all cost? Who pays for it, and what is the use of it?" The main question, however, to which he applied himself, was: "How can they be attended to?" and his answer gives a vivid picture of the machinery of the old casualty department and its work forty-five years ago. Those who performed the feat each morning of clearing the hall of medical casualty patients consisted of Dr. Bridges and two other casualty physicians, and the junior assistant physician, who each attended four mornings a week. Among the problems of a casualty physician was "to weigh the inconvenience of overcrowding the hospital dispensary against the lesser, or at least less visible, inconvenience of treating a number of different complaints with the same medicine; but what stands most in his way is no doubt his want of time. . . . And yet filter he must, and filter quickly too, and be prepared to hear his quick filtrate shamefully ill spoken of in the wards and in the out-patient rooms. . . . He will decide at once that what he has to do is the best that can be done for every individual case under the circumstances, and he will make the best of a bad business, and take it patiently." Feeling thus the responsibility and difficulty of the task, Dr. Bridges made a system of hieroglyphic notes for checking off each case as he dismissed it; these records formed the statistical basis of his paper. At that time he saw 7,735 patients in three months, or some 30,000 patients in the year, and of these more than two-thirds were new cases. In the three months nearly 2,000 of his patients took quassia and iron, affectionately known as "Queen Anne" mixture —"a medicine which I selected to give as frequently as possible in consideration of its strong taste, cheapness, and innocuous properties." Allowing for the exiguous sum paid to the casualty physician, it appeared that the total cost of his "Queen Anne" patients was well under twopence a head. Dr. Bridges found that in the year he took book-notes of only 70 cases, and reflected on how much rich material for study was daily thrown away. The congested casualty department in his time supplied less than a quarter of the cases in the wards, and he came to the conclusion that the serious cases bore no relation to the trifling ones, and that the former would come in just as great numbers were the latter discouraged. While agreeing that the mistakes were very rare and the treatment fairly satisfactory, he maintained that the system was intolerable and must be reformed. How far reform has gone in forty-five years might be a congenial subject for inquiry by one of our younger medical poets. Great changes were made for the better when the new casualty and out-patient department of St. Bartholomew's was opened in 1907.

[1] *St. Bartholomew's Hospital Reports*, vol. xiv, 1878, p. 167.

BRISTOL MEDICAL SCHOOL.

WHEN Bristol University College was founded in 1877 some of the lecturers in the Medical School—among whom Dr. Shingleton Smith may be specially mentioned—were anxious to see it incorporated completely with the College. This was not found feasible, but the School was affiliated with the College, and after a few years medical students in the preliminary subjects, up to and including anatomy and physiology, received their instruction in University College. This plan was continued when the University was founded in 1909, but complete incorporation was still desired, and this has now being accomplished. The clinical portion of the students' curriculum, which heretofore has been a proprietary affair of the honorary staffs of the Royal Infirmary and General Hospital, has been given over to the University, and the students' fees will be paid in to the University exchequer. The only private right the staffs retain is the privilege of introducing pupils to their hospital practice. This right is derived from a stipulation made between the original founders of the Bristol Royal Infirmary and the first surgeons elected to that institution, whereby the surgeons undertook to give gratuitous service on condition that they were allowed to introduce their apprentices to the wards of the Infirmary. The Bristol Medical School was the foster-mother of University College, and out of the College grew the University; the new arrangement by which the Medical School becomes an integral part of the University cannot fail to strengthen both. The surrender of the management of clinical teaching and the disposition of clinical fees is a big step, one effect of which will be to influence the Government grants to the University. Hitherto the Treasury has not taken into reckoning "clinical studies" in medicine as part of the functions of the University. In a recent communication to the University the University Grants Committee stated that it disapproved of "proprietary" medical schools and would make no grants in aid of them. The staffs of the Infirmary and Hospital became convinced that a change was desirable, and by a unanimous decision clinical teaching and clinical fees have been made over to the University of Bristol as from August, 1922. The University in return has arranged a scale of payments to clinical teachers which, although small, will have the advantage of not fluctuating with the number of students, and moreover is designed to remunerate teachers on the basis of work done—a plan which the old system did not ensure. The new arrangement has led to the appointment of three directors of clinical studies, whose chief duties will be to co-ordinate the teaching at the two institutions, and to organize on a coherent and continuous basis the clinical instruction of medical students. The directors appointed are Professor Edgeworth in medicine, Professor Hey Groves in surgery, and Professor Swayne in obstetrics and gynaecology. These newly appointed directors start with the goodwill of all their colleagues, and the scheme appears to have won the approbation of the students.

SACRAMENTAL HEALING AND THE PSYCHOLOGY OF CONVERSION.

AT the Church Congress which was held in Sheffield last week questions of lively medical interest did not figure so largely as at the last congress, when Lord Dawson spoke on birth control; but the subjects of sacramental healing and of the psychology of conversion came in for some discussion. Dr. Chandler, lately Bishop of Bloemfontein, summed up the Church system of therapeutics as consisting of the sacraments or sacramental rites of confession, unction or the laying on of hands, and communion, and in that order. He said that doctors engaged in mental therapeutics had made the significant criticism of sacramental methods of healing, not that such healing was illusory, but that it was superficial, dealing only with outward symptoms, and not penetrating to deep, underlying moral causes. In many, though not all, cases of functional nervous disorder there was an underlying moral cause, a repressed complex resulting from some moral struggle, and he believed that this cause ought to be dealt with, before the

physical symptoms were touched at all, by a skilled confessor. The priest in confession ought to be able to supply, with equal effectiveness and under more rigorous safeguards, the services which the psycho-analyst aimed at rendering. It was claimed for psycho-analytical treatment that it differed from spiritual direction in securing a more complete and radical cure, so that no repetition of the treatment was needed. He believed that this view could hardly be maintained. It might hold good in certain sudden and violent emotional shocks where the struggle between instinct and ethical code had driven the instinctive reaction immediately underground and produced a neurosis; in such cases, of which war shock was a typical instance, the reassociation or breaking into consciousness of the repressed complex might be all that was required to re-establish the normal. But in the more usual cases there was necessity for repeated treatment, either in the way of precaution or cure, and this was best done by the sacrament of confession. Afterwards the physical symptoms might well be treated more directly by the further sacramental rite of unction or of the laying on of hands. The reconsecration of the body to the Divine will which was thus effected came as a natural and proper sequel to the absolution previously received. Finally, the whole nature of the man, spiritually and bodily, was strengthened and built up by the sacrament of communion, which aimed at preserving body and soul unto everlasting life. Bishop Chandler claimed that in this way the sacraments, uniting body and soul together had their part to play in the cure of functional disorders although their primary aim was the spiritual good of the sufferer. The psychology of conversion was the subject of another discussion opened by Dean Inge, who criticized the view that conversion was always or even usually a sudden event rather more vigorously than some sections of his audience relished. Dr. William Brown, Wilde Reader in Mental Philosophy at Oxford, who followed, argued that conversion was not primarily psychological; it was religious, which meant that it belonged to a realm beyond the reach of psychology. He held that psychology could not explain the distinctive religious experience, and had to do only with the pathological. It could explain deviations, errors, and evil, but it could not explain normal appreciations, truth, or goodness. He believed that it was better for conversion, or the turning away from the naturalistic to the religious, to come gradually, but there were certain temperaments in which the gradual process was impossible. In these cases conversion was frequently preceded by melancholia, which was an outward and visible sign of maladjustment and of unsolved conflicts in the subconscious. The fact that the mystical experience came suddenly did not mean that it was a pathological experience, although in the cases of sudden conversions the danger of pathological characters linking themselves on to the normal process was greater. He was in profound disagreement with the psycho-analytical theory which explained religious experience as a regression to an infantile attitude of mind. There were those who said that when a person became converted he regressed to his childhood, and early tendencies, memories, and fantasies assumed for him an objective reality, although they were only a re-edition of his early experience. In Dr. Brown's view this was far too shallow a theory, and, as with so many other aspects of psycho-analytical doctrine, it was put forward without having been tested by people thoroughly trained in psychology and in philosophy.

THE ABORTIVE TREATMENT OF SYPHILIS AND SALVARSAN RESEARCH IN GERMANY.

PROFESSOR KOLLE,[1] on whom the mantle of Ehrlich has fallen, has been engaged for a considerable time in studying the effects of salvarsan on experimental syphilis in rabbits. The two questions he set out to answer were: (1) Is it possible, and if so in what percentage, to sterilize with salvarsan rabbits infected with syphilis? (2) To how late a stage after infection is sterilization feasible? Kolle agrees with Finger, Landsteiner, Uhlenhuth, and others, that once a rabbit has been infected with the Truffi strain of the *Spirochaeta pallida* it is immune for the rest of its life. This immunity to reinfection from without in the form of a typical primary chancre is conditional on its not having been given any antisyphilitic treatment, and on ninety days or more having elapsed since the first infection. Among the many hundred rabbits he has inoculated he has not seen a single exception to this rule, although in a few cases he has observed atypical infiltrations or ulcers, containing spirochaetes, at the site of a second inoculation. Within the first sixty days of the first infection he has been able to induce reinfection in 50 to 60 per cent., and he has achieved this result in a few instances between the sixtieth and ninetieth days. Within 3, 15, 20, 25, 30, 45, 60, 90, and 120 days of the first infection rabbits were given three large doses of salvarsan preparation, and attempts were made 110 to 120 days after the first infection to induce a typical chancre by reinfection. Various salvarsan preparations were used, and neo-silver salvarsan figures most prominently in Professor Kolle's tables. Changes were also rung with mercury in combination with salvarsan, and the development of a typical chancre after reinfection was accepted as evidence of effective sterilization of the tissues after the first infection. It was found that when this sterilization was attempted within the first forty-five days of infection complete recovery occurred in a considerable percentage, whereas after the forty-fifth day this was the exception. In no single instance in which salvarsan was administered after the ninetieth day did a typical chancre develop when an attempt was made to reinfect. There is thus weighty experimental evidence in support of the clinical finding that the success of salvarsan medication depends largely on the interval between infection and treatment being short, but it is, of course, doubtful whether the forty-five day limit in the case of the rabbit can be accepted as the "sterilization limit" for man. Another interesting observation made by Professor Kolle was that the combination of mercury with salvarsan did not add to the number of his successes, and he claims that this is in accord with clinical experience. The dosage of salvarsan recommended by Professor Kolle for man is 0.45 gram of neo-silver salvarsan, 0.3 gram of silver salvarsan, or 0.5 to 0.6 gram of neo-salvarsan, these doses being approximately two-thirds of what he calls the *dosis tolerata*. He also recommends the injection of a precautionary dose of 0.05 gram of salvarsan twenty-four hours before the main injection is given, and the addition of a 40 per cent. solution of sugar (glucose) to the salvarsan. It will be seen that Professor Kolle has reverted to his predecessor's scheme for a *sterilisatio magna*, although he does not use this term in his paper, and he recommends not one but three to four large injections. He doubts whether the percentage of biological cures can be appreciably increased by a greater number of injections. Investigations are proceeding in his laboratory in connexion with the therapeutic possibilities of prolonged and intermittent treatment of fully established syphilis with salvarsan alone or in combination with mercury, but they are not sufficiently advanced to allow any conclusions to be drawn from them. With regard to the early abortive treatment of syphilis with large doses of neo-silver salvarsan, however, he is very optimistic.

[1] *Deut. med. Woch.*, September 29th, 1922.

AN INTERNATIONAL CONFERENCE ON TAR CANCER.

AN international conference of those engaged in the experimental study of cancer opens at Amsterdam on October 23rd, and will continue until October 25th. It has been arranged by the Netherlands Cancer Research Society, and Professor Rotgans, Emeritus Professor of Surgery in the University of Amsterdam, will preside. Papers will be contributed by Professor Fibiger and Dr. Bang of Copenhagen, Drs. Archibald Leitch and J. A. Murray of London, Dr. Deelman of Amsterdam, Dr. Bierich of Hamburg, Dr. Teutschlander of Heidelberg, Dr. Lipschütz of Vienna, Professor Bloch of Zurich, and Drs. Peyre, Roussy, and Leroux of Paris. Most

of the papers will deal with the various phenomena observed in the production of cancer by tar. Dr. Murray will contribute a paper on the criteria of malignancy in experimental cancer, and Dr. Archibald Leitch will read three communications on the effect of cessation of the irritant on the development of tumours, and on the experimental production of paraffin cancer and arsenic cancer. At the end of the conference Professor Fibiger will deliver an address on recent investigations in cancer.

A GOOD OPENING.

Those who have to look through the annual reports of public institutions are not always rewarded for their pains. But now and then a chronicle of dull but useful events is enlivened by a note of criticism, or pages of statistics are prefaced by a few words indicating an outlook wider than the institution grounds. The effect upon the committee must depend largely on how the thing is done. An introductory note to the last annual report by the Medical Superintendent of the Kent County Mental Hospital at Barming Heath has, we suspect, been appreciated. "During the year [he writes] mental disease and its treatment have been much in the public mind. Psycho-analysis nowadays is freely discussed in popular magazines; the man in the street, not a little bewildered by the jargon of the psycho-analyst, ponders on the properties of the subconscious mind, and tries to recall the detail of his dreams. Monsieur Coué, the philosopher from Nancy, draws crowded audiences to his lectures on auto-suggestion, and the daily papers record the remarkable cures that occur at his séances. There is indeed nothing new under the sun! The interpreters of dreams were ever held in esteem by the ancients. Buddha, Mahomet, the Shrine at Lourdes, the King's Touch, are but a few of the agencies that have effected cures by auto-suggestion. It is not generally recognized that the emotions are more powerful than the intellect, and that the imagination is stronger than the will." Next follow a word or two about *The Experiences of an Asylum Doctor*, and the advantage of ventilating a matter which in the past has been too much "wropt in mystery"; a hint on the need of better opportunities for training the medical and nursing staffs of the great mental hospitals; and a suggestion that greater facilities should be given for treating mental disorder in its earlier stages. Having thus whetted his readers' interest, Dr. Wolseley-Lewis proceeds to the record of a year's work, mindful perhaps of Bacon's advice to negotiators: "A man may not look to sow and reap at once; but must prepare business, and so ripen it by degrees."

THE VITAL STATISTICS OF ONTARIO.

According to the last annual report on the vital statistics for the province of Ontario, Canada, the estimated population in 1920 was 2,895,370. The distribution of the population was as follows: the cities contained 54 per cent., the towns with a population of over 5,000 5.8 per cent., and the rural municipalities, villages, and small towns 40.2 per cent. The birth rate for the whole province was 25, and the death rate 13.9 per 1,000 of the population. The chief cause of death during the year was influenza, which accounted for the deaths of 113.1 persons per 100,000 of the population. This was due to an epidemic during the autumn and early winter of 1919–20, which was, however, not so widespread and virulent as the previous epidemic of 1918, when the extraordinary death rate of 262.2 per 100,000 of the population resulted. After influenza the most important causes of death (omitting pneumonia, which was doubtless influenced by the influenza epidemic) were organic heart disease, cancer, and tuberculosis. The deaths from tuberculosis numbered 2,280, a rate of 78.7 per 100,000 of the population. The cities had a rate of 74.7, the towns of over 5,000 population 85.7, and the rural municipalities 81 per 100,000 of the population. The campaign against tuberculosis has received assistance from municipal activities and from voluntary societies, while useful educational work has been done by the Canadian Association for the Prevention of Tuberculosis. It is suggested in the report that tuberculosis is one of the diseases in the prevention of which the Dominion Government should afford greater assistance to the provinces. The entire cost of tuberculosis work in Canada, with the exception of 10,000 dollars voted to the Canadian Association, is borne by the provincial governments. In regard to cancer, the number of deaths for the year was 2,464, giving a rate of 85 per 100,000 of the population, as compared with 16 in 1881. As elsewhere, the apparent increase in cancer mortality may be accounted for to some extent by improvement in diagnosis and more satisfactory death records.

MENTALLY DEFECTIVE CHILDREN.

The Birmingham School Board appointed a Special Schools After-care Committee as early as 1901; the Committee still exists, but for the after-care of the mentally defective only. The annual report for 1920 deals with the work of this Committee. During the past year 2,655 cases have been under care. Of these, 707 are doing remunerative work, 76 were dismissed as incapable, 625 are living at home and doing no paid work, 163 improved so much that they could be transferred to ordinary schools, 25 went to schools for physical defectives (mostly to schools for the deaf), and 271 now live in institutions for the defective. No less than 480 (25 per cent.) have been lost sight of, and most of them before the age of leaving school. The percentage of those in employment has fallen from 45 per cent. in 1920 to 32 per cent. during the past year. This is the lowest percentage of employed cases since 1912, but it is considered good under the abnormal conditions of trade, which affects these young persons first. The report shows a small increase in the marriage rate; there have been five new instances of the birth of illegitimate children, and several girls are said to be leading irregular lives. The proposed limitation of provision for these mental defectives after school years under the economy circular of August, 1921, led to the production of conclusive proof that it was far more economical to the nation as a whole to maintain certain types of mental defectives in suitable institutions than to be dealing with them periodically in prisons, workhouses, or maternity homes. The circular which proposed the reduction of accommodation was eventually withdrawn. Cases are cited which show the great advantage of the segregation of the dangerous mental defective. The Committee has under consideration the provision of a workshop where employment could be found for a certain number of those who easily fall out of work and yet are able to do something.

INTERNATIONAL ASSOCIATION OF MEDICAL WOMEN.

A meeting of the London Association of the Medical Women's Federation was held on October 10th with the president, Dr. Louisa Martindale, in the chair. Lady Barrett, M.D., gave a report of the international meeting of medical women which was held in Geneva last month, at which eighty medical women were present, representing eighteen countries. (A report appeared in the Supplement of September 16th, p. 111.) Lady Barrett stated that the idea of an international association of medical women had originated simultaneously here and in America. The Medical Women's Federation here had hoped to inaugurate an international association which should be composed of national associations, while in America an international association had already been started in 1919, composed of individual members from many countries of the world. At the request of the Americans the Medical Women's Federation of Great Britain drew up a draft constitution, which was to be presented to the Geneva Conference of 1922. The delegates from the Federation to this Constitution Committee were: Dr. Jane Walker (chairman of the committee at Geneva), Dr. Catherine Chisholm, Dr. Christine Murrell (joint secretary to the committee and vice-president of the American International Association, 1919), Dr. Dickinson Berry (joint secretary to the committee), Lady Barrett (president of the British Federation). Lady Barrett said that there was considerable difference of opinion at the conference as to the basis of membership, but

that finally a compromise was reached. It was decided that membership should be on the basis of national associations of medical women, in some cases sections of medical women, and in a few instances individual medical women should be eligible.

SIR W. LESLIE MACKENZIE, M.D., LL.D., has been appointed for five years as the Crown representative for Scotland on the General Medical Council in succession to Dr. J. C. McVail, whose term of office expires on October 28th.

SIR ALFRED KEOGH, G.C.B., G.C.V.O., M.D., has been awarded the gold medal of the Institution of Mining and Metallurgy "on the occasion of his retirement from the rectorship of the Imperial College of Science and Technology, in recognition of his great services in the advancement of technological education, and as a mark of admiration and respect."

MOTOR NOTES FOR MEDICAL MEN.

BY H. MASSAC BUIST.

TAXATION.

The "Agreed" Recommendations.

THOSE motoring organizations that have been in consultation with a view to the production of what may be called an agreed, or unified, recommendation in regard to the recasting of the taxation scheme in respect of the ownership of motor vehicles in this country have sent their report to the Ministry of Transport. It is a document with which the members of the medical profession are very intimately concerned. The recommendations are on behalf of the Society of Motor Manufacturers and Traders, the Scottish Motor Trade Association, the Motor Agents' Association, the Motor Trade Association, the Association of British Motor Manufacturers, the Institute of British Carriage Manufacturers, and the British Cycle and Motor Cycle Manufacturers' and Traders' Union, as well as of the Royal Automobile Club, the Royal Scottish Automobile Club, the Automobile Association and Motor Union, and the Auto-cycle Union, which four bodies, it is asserted on the cover of the memorandum, represent "all classes of motor vehicle owners in Great Britain." For a point of some accuracy it will be observed, however, that the Commercial Motor Vehicle Users' Association is not represented. That is an organization the members of which own some tens of millions of pounds' worth of road stock. The utility motor vehicle users of this country will have nothing whatever to do with the recommendations of the report. It is suggested that the registration book has failed to serve any useful purpose, and that it should be abolished, thereby saving a proportion of present administration costs; with this, doubtless, all will agree. It is further suggested that annual car licences should be issued to expire on any date; this, again, is a reform greatly needed. Another recommendation is that, with the proposed reduction of the registration tax, there would be little incentive to take up quarterly or monthly licences, and that this would result in further administrative economies. Other recommendations are:

(*a*) A flat rate duty per gallon to be imposed on all imported motor spirit to provide the bulk of the required revenue, the remainder to be derived from a reduced tax on the vehicle.
(*b*) All imported liquid hydrocarbons to be taxed at the port of entry on "motor spirit content" (the term is defined in Part 4).
(*c*) Motor spirit for motor boat, aviation, or any purposes other than for motor vehicles, to be supplied duty free on a permit system, such spirit, if desired by the authorities, to be coloured suitably.
(*d*) No duty to be imposed upon motor spirit obtained from raw materials produced in Great Britain.
(*e*) No rebates of duty to be granted to any user of motor spirit.
(*f*) Vehicles, other than those with internal combustion engines, to be taxed on the present method.

It is noted that any proposal submitted must be such as to secure a revenue equivalent to that produced in 1921–22, exclusive of the receipts in respect of licences for horse-drawn vehicles, drivers' licences, and penalties. Nothing is said as to the cost, nor are specific details furnished concerning what is called a simple permit system whereby fuel for motor boats, aviation, and such-like purposes, is proposed to be duty free. Nor is the very practical question, always asked on behalf of the Excise authorities, answered; yet it was understood that practical politics was to be the sole purpose of these negotiations. Frankly, I am extremely disappointed that there is no specific constructive paragraph showing how the Excise authorities are to solve their problem in respect of motor spirits distilled from crude oils imported into this country. Benzol, and such-like motor spirit produced in this country, is to be duty free. But under this head I fail to discover any solution of the problem of the lucky *versus* the unlucky motor vehicle owner. For example, according to this system, the man who secures home-produced motor fuel, which would be duty free, would benefit; but those who resided in neighbourhoods where such supplies were not available would be among the unlucky ones who were paying for the roads which "the other fellow" would be enjoying practically free of cost to himself. When it comes down to practical details, there are only two schemes, submitted on page 11 of the memorandum, and both concern approximate revenue. According to scheme A, the motor spirit duty would be at the rate of 5d. a gallon, and the registration duty 2s. 6d. per horse-power.

Horse-power Rating would remain Unaltered.

We know that the Treasury rating of horse-power would continue as at present, because the Society of Motor Manufacturers and Traders has discussed that matter, and has decided that no useful purpose could be served at this juncture by altering the basis of the Treasury rating, no matter how unsatisfactory it may be, since we should only leave the ills we have to fly to others that we know not of. Under scheme B in the report, the motor spirit duty would be 4d. a gallon, and there would be 75 per cent. reduction on the horse-power tax, which would be approximately 5s. per horse-power. Under both these schemes the man who secured benzol, or other home-produced fuel, would escape respectively 5d. and 4d. a gallon duty. Whereas when last a fuel tax was in vogue medical men were exempt, it is specifically laid down in these recommendations that there shall be no rebates nor exemptions other than those already specified under the reference permits. Inasmuch, too, as the ratio of horse-power tax that would be paid by medical men owning various classes of vehicles would be just the same as that which, it is alleged, is generally complained of at present, it would follow that the owner of a Ford car, for example, by comparison with the owner of, let us say, one of the new small 8-h.p. (Treasury rating), 4-cylinder, overhead valve, small cars, would be at the same disadvantage as he labours under at present, because Treasury rating would be the basis of both rates of duty. Further, the economy of the modern British or French built vehicles under the head of fuel, by comparison with the Ford, would also continue to give the advantage to the product that represented the latest higher standard of engineering efficiency. Moreover, it is frankly admitted on behalf of the recommendations that a large number, including small-car owners, would pay considerably less under the net heading of taxation than they do at present; and that, inasmuch as the gross amount of revenue to be reaped by the Treasury must be the same, those who used their cars considerably more than, say, 3,000 miles a year would have to make up the difference. Being interpreted, that means that the members of the medical profession in general would contribute more than any other section of motor users towards relief proposed to be accorded. The scheme represents, incidentally, a return from a single tax, as at present, to a dual tax. Take it for all in all, though it is called an agreed scheme, it must not be understood for a moment that it represents the views of all the members, or even necessarily a majority of the members, of the organizations that jointly put it forward. Not more than one member in three of the A.A. has troubled to back it by referendum. Certainly it will meet with much opposition.

FUEL, TYRE, AND CAR COSTS.

A great change has come over the spirit of motoring during the autumn. Fuel costs have come down from 5½d. a gallon, not owing to any immediate possibility of selling more petrol during the winter months at a lower cost, but chiefly because long-term contracts in respect of shipping and such-like costs contracted for at the height of the post-war boom have now lapsed; therefore matters are getting back towards normal. An even more important factor is the great increase in the market for the residuary products of

the distillation of crude oil. This growth of the fuel oil market, largely for the service of the world's shipping industry, is something which motorists have long had cause to desire; hitherto they have had to pay more for their petrol because the manufacturers were loaded with residuary products which had to be sold at a loss, at no profit, or at an inadequate profit. Therefore, the more the markets for the residuary products of crude oil distillation can be developed the lower will be the proportion of the total cost of engaging in enterprise of this sort, which has to be borne by the users of the more volatile products, motor spirit in particular.

Tyres Cheaper than before the War.

Simultaneously with this material cheapening of petrol, which really represents a reduction of the fuel bill by one-sixth, there have been notable reductions in tyre costs. The result is that treads to-day of post-war construction are marked by the best makers at about the same figure as obtained in 1914, with this notable difference, that for these corresponding sums—there is not 3s. 6d. per average size of tread difference—the post-war production is designed on such lines and made by such infinitely superior manufacturing methods—including, notably, the curing of rubber—that the tyre mileage obtainable to-day is at least two and a half times as great as was possible in 1914, and in most cases three times. The actual result depends very much less on luck than formerly. But, of course, it still depends largely on the manner in which the individual drives, alike as regards changing gear and the application of brakes, as well as in respect of his care in not running his tyres on to the points of tramlines, and so forth. Assuming he drives with judgement, however, the medical man should have at least two and a half times the tyre mileage with corded type covers that he had before the war with the fabric types then available. The vast progress that has been made in the curing of rubber since 1914 will be appreciated by anyone who will follow my example and visit the laboratories instituted for rubber research. The system of construction is better and the product much more durable. The inner tubes, now sold by most of the leading makers at approximately half the price at which they were marketed in 1914, are better tubes. Therefore, under the heading of tyre costs the medical man's motoring bill is reduced from now onwards to a figure lower than any he has previously experienced. An illustration may be taken from the Dunlop Company's list, citing the popular size tyre, 815 by 105. When the inferior canvas cord groove type only was available in 1914 the cost was £5, whereas to-day the Dunlop clipper cord type, which is a better article, costs £4 13s. This year's price for this size Dunlop cord type of the same size has been £7 2s. 9d., whereas to-day it is £5 3s. 9d. only.

Insurance and Capital Outlay.

In regard to motor cars, while the Paris show has been held, nevertheless it is a fact that we shall not realize fully what motor car marketing enterprise amounts to in the gross for next year until the opening at Olympia, Kensington, and the White City, Shepherd's Bush, London, on November 3rd, of the International Passenger Car Show, of which I propose to give some account in the JOURNAL of November 4th. A large proportion of the chassis, and of even complete cars, staged in the Grand Palais in Paris this month were the identical vehicles which were shown there last year. By contrast, there will scarcely be an exception to the rule that this autumn's products alone will be staged at the London exhibition. Further, the reductions in the cost of labour and of raw materials, and progress alike in design and in manufacturing methods and equipment, have rendered possible great reductions in the cost of cars. Apart from the cost of garaging, the other chief items of cost in respect of motoring are insurance and taxation. Taxation is referred to above. Apparently the insurance rates are not likely to fall as have fuel, tyre, and car costs, but, inasmuch as cars are to be marketed at very much lower prices, capacity for capacity, it follows that the actual sums of money on which insurance will have to be paid will be reduced automatically and appreciably in respect of those who acquire cars from now onwards. But insurance rates must continue to rule high as long as average driving results inevitably in the high proportion of claims current; and there is as much car stealing as has been experienced since the war. If we can reduce these adverse factors we shall find notable reductions in the cost of insurance.

England and Wales.

NATIONAL MILK CONFERENCE.

A NATIONAL MILK CONFERENCE was held at the Guildhall, London, on October 16th, 17th, and 18th. It was called by the National Clean Milk Society, supported by certain other bodies, including the British Medical Association (which was represented by Dr. T. W. H. Garstang), the Royal Institute of Public Health, the Royal Sanitary Institute, and the Federation of Medical and Allied Societies, as well as a number of agricultural organizations. On the first day of the Conference the matters under discussion were in the main of agricultural interest, such as the breed of cattle in relation to quantity, chemical composition, and cost of milk production, and the feeding of dairy cattle in relation to this last factor. On the subsequent days discussions took place on bovine tuberculosis, the chemical composition of milk, pasteurization, and food values. The Conference was attended by some two hundred representatives, including several visitors from the dominions and foreign countries, three or four of whom came as representatives of their Governments.

Lord Astor, in an opening speech, declared that neither the milk industry itself nor the Ministry of Agriculture had in the past realized the importance of milk as a beverage. Uncontaminated milk was as important, in view of the health of children, as an uncontaminated water supply. Every year the milk trade lost thousands of pounds owing to premature souring, which was the result of uncleanly methods. Sir A. Griffith-Boscawen, Minister of Agriculture, who took the chair at the first session, said that the nations which consumed most milk per head of population were the most virile, and the consumption of more milk per head would be a great advantage to this country. A later speaker gave figures to show that in New York twice as much milk was consumed per head as in London. Among the papers read on the first day one was by Sir Stewart Stockman, chief veterinary officer and director of veterinary research at the Ministry of Agriculture and Fisheries, who dealt with epizootic bovine abortion and its effect upon milk supply. This contagious disease of cattle, due to a non-motile bacillus, has been specially studied at the Ministry's veterinary laboratory with a view to elaborating a method of protective inoculation, for which good results are claimed. In an area where the disease was prevalent, out of 493 test animals vaccinated 32 (or 6.5 per cent.) afterwards aborted, while of 265 animals not immunized, and left as controls, 101 (or 38 per cent.) aborted. In reply to a question, the speaker said that there was no evidence whatever to support the view that if milk which happened to contain the abortion bacillus was drunk by pregnant women it might lead to human abortion; tests had been made with the blood of pregnant women, and no sign of any agglutination reaction had been discovered.

The remaining subject at the first session was the production of clean milk, in particular the education and equipment of the dairyman. Dr. R. Stenhouse Williams, Research Professor in Dairy Bacteriology, University College, Reading, in opening the discussion, described the clean milk campaign, which had made considerable progress in some parts of England. More "Grade A" milk from tuberculin-tested cows was sold in Reading than in any other town in the country. There were five centres in Reading in which this milk could be secured, at one penny a quart above the ordinary price. Another town well advanced in the movement for clean milk was Leicester, where the authorities had announced that no contracts for milk for hospitals or schools would be accepted unless the milk was "Grade A," for which an extra threepence a gallon would be paid. The speaker feared the effect of Government regulation upon the clean milk movement. It seemed likely that, following on recent legislation, "Grade A" licences would be granted simply on veterinary inspection of the cattle, so that the "Grade A" farmer who resorted to the tuberculin test would get no official credit for the work he had done. Mr. Wilfred Buckley, C.B.E., who was presiding over the Conference at the time, said that no official statement had been issued by the Government as to the nature of the forthcoming regulations, and he understood that the Ministry of Health had under consideration the requirement of the bacteriological count as well as the veterinary inspection. Some sharp divergence of opinion was manifested by various medical officers of health who took part in the subsequent discussion as to the extent to which contamination of milk occurred in the home. Dr. Nash denied emphatically that the

home was responsible for any considerable part of the contamination.

A report of the later sessions, over which Sir Alfred Mond, Viscount Elveden, Sir Walter Fletcher, and Sir George Newman presided, will appear in our next issue.

NEWCASTLE-UPON-TYNE AND NORTHERN COUNTIES MEDICAL SOCIETY.

The first meeting of the session was held in the Royal Victoria Infirmary on October 5th. Professor Stuart McDonald, Vice-President, was in the chair, and about a hundred members were present. The first part of the meeting was a demonstration of cases and pathological specimens. Cases were shown by Drs. W. E. Hume, Horsley Drummond, George Hall, Messrs. H. Brunton Angus, S. S. Whillis, R. J. Willan, and Hamilton Drummond. The pathological specimens were arranged by Mr. W. E. M. Wardill, curator of the museum of the Durham University College of Medicine. Later in the evening a most interesting paper was read by Mr. F. C. Pybus on "Some surgical aspects of dyspepsia." The paper will appear in full in the *Newcastle and Northern Counties Medical Journal*. During the meeting the sad news was received of the death of the President of the Society, Dr. Lachlan G. Fraser, and a vote of sympathy with the widow and family was passed. The Society meets monthly during the winter session, and in addition to the ordinary meetings two clinical and pathological meetings have been arranged for the convenience of country members at 4.30 p.m. on October 25th and January 24th. The annual address is to be delivered on December 7th by Sir Humphry Rolleston, President of the Royal College of Physicians of London, on "Reflections on high arterial blood pressure." The annual dinner will be held the same evening, when Sir Humphry Rolleston will be the guest of the Society. On February 1st an address on "Diagnosis in surgery" will be given by Mr. D. P. D. Wilkie of Edinburgh.

LONDON HOSPITAL DINNER.

The London Hospital Old Students' dinner was held at the Trocadero Restaurant on October 12th. The chair was taken by Sir Arthur Keith, F.R.S., and he was supported, amongst others, by Lord Knutsford (Chairman of the Hospital), Sir Hugh Rigby, Sir John Lynn-Thomas, Sir William Lister, Sir James Dundas-Grant, Mr. T. H. Openshaw, Mr. James Sherren, and Dr. Robert Hutchison. The principal guest was Surgeon Vice-Admiral Sir Robert Hill, K.C.B., K.C.M.G., Medical Director-General of the Navy. The gathering was large and enthusiastic, each generation of old students being well represented, while the hospital staff turned out in force. If one factor more than another could be held responsible for the success of the evening, that should be the method adopted in arranging the tables. So far as possible each table was given up to one generation of old students, so that it was practically certain that any man attending the dinner, even if he had lost touch with his old school for years, must find himself seated among old friends. To the honorary secretaries, Messrs. Souttar and Lindsay, more than an ordinary tribute of praise is owing for the art displayed in so skilfully arranging such a numerous gathering. As should be on an occasion such as this, the evening was devoted more to the renewing of old friendships—the harking back to the good old days—rather than to listening to a number of set speeches. After the toasts of "The King" and "Queen Alexandra, Patron of the Hospital" had been duly honoured, Sir Arthur Keith proposed "The London Hospital." In a humorous manner he explained the circumstances which brought about his appointment many years ago to be senior demonstrator of anatomy at the princely salary of £100 a year. In happy vein he developed his theme to show that not only was the research worker starved by the totally inadequate reward offered for his labours but the aspirant to the hospital staff was similarly handicapped in his early years. He had to shoulder an utterly unfair burden in being forced to devote so much of his time to the gratuitous service of his hospital. Not only had the staff themselves to carry a share of the burden of sickness in London, they had at the same time to satisfy the by no means modest claims of the West End landlord. He appealed to the wealthy citizens of London to give generously and freely towards the endowment of medical education that the perpetual war against disease might be waged successfully by the combined efforts of clinical and laboratory research workers. The toast was supported by Lord Knutsford and by Mr. Openshaw, who said that no man had so closely followed in the footsteps of John Hunter as had Sir Arthur Keith. The health of the chairman was briefly proposed by Dr. Robert Hutchison.

KING'S COLLEGE HOSPITAL WAR MEMORIAL.

On October 11th Viscount Hambleden, chairman of the hospital, declared open a war memorial pavilion on the athletic ground of King's College Hospital Medical School. The tablet he unveiled bears the inscription, "Dedicated to the memory of the Students of this School who fell in the Great War—1914–1918." The history of the war memorial was recounted by Mr. L. V. Cargill, senior ophthalmic surgeon. Lord Hambleden in the course of his address said that of all the wonderful organizations created during the war none were more remarkable than the medical services of the two great fighting forces. The services of the medical men who joined the armed forces could never have been rendered had it not been for the work of the voluntary hospitals and their medical schools in former years. "We appreciate to the full," he said, "the work of the Regular officers of the R.A.M.C. and the powers of organization which they showed. They called upon, and not in vain, the services of men who had been trained for ordinary civilian work, who turned their knowledge and capacity to the service of their country in the war. We can surely be proud as King's College men of the part which old students, our honorary staff, and our nurses played, and especially are we proud of those who made the greatest sacrifice of all and gave their lives to the cause of their country and their fellow-men." A vote of thanks to Lord Hambleden was proposed by the Dean, Dr. H. Willoughby Lyle, and seconded by Dr. H. A. Lucas.

MIDWIVES AND CHLOROFORM.

A special meeting of the Central Midwives Board for England and Wales was held on October 11th, Sir Francis Champneys presiding. Three cases adjourned for reports and eight fresh cases were dealt with; six midwives were removed from the roll. At the ordinary monthly meeting, which was held on October 12th, a letter was read from the clerk of the Middlesex County Council reporting that the County Council was not in a position to find a prima facie case under Section 8 (2) of the Midwives Act, 1902, against a midwife who had administered a "whiff of chloroform," and again asking for the Board's views as to the desirability of a midwife administering chloroform. It was agreed to reply as follows:

> "That the Board will express its opinion as to whether or not the use of a drug by a midwife is justified when it has a concrete case before it, and is aware of all the circumstances attending the administration or the use of such drug. The Board sees no reason why the midwife should not be reported in order that the Board may consider the question on a concrete case."

Certain lecturers and teachers were approved, and the meeting terminated.

Scotland.

PRESENTATION TO DR. J. R. CURRIE.

DR. J. R. CURRIE, of the Scottish Board of Health, was entertained to dinner in the hall of the Royal College of Physicians, Edinburgh, by a number of medical colleagues on the occasion of his impending departure to take up the duties of professor of preventive medicine in Queen's University, Kingston, Ontario. Dr. John Playfair presided, and the company included Sir James Hodsdon, Sir Leslie Mackenzie, Sir Robert Philip, Dr. Norman Walker, Dr. A. K. Chalmers, Dr. J. R. Drever. Sir Robert Philip proposed the health of the guest, and Dr. Currie, in reply, said that the work of a medical officer had changed greatly in the ten years since he was medical officer for the counties of Fife and Kinross. At that time the medical officer dealt with the purity of streams and water supplies, removed cases of infectious disease to hospital and dealt vigorously with infection, and was very wise on the subject of drains. Nowadays tuberculosis alone had broadened into a special subject by itself; they had added the hygiene of the young child to the hygiene of the school child, and included the hygiene of the infant and the child unborn. Preventive medicine should go on and take a grasp of the social questions of the hour and of the future, and it was now said that all medical teaching should have a preventive bias. Dr. Norman Walker, in

presenting Dr. Currie, on behalf of his Scottish friends, with a silver bowl and a silver tea service, commented upon the success of the work which Dr. Currie performed in connexion with the Scottish Emergency Committee on medical service during the war.

Links between Natural History and Medicine.

The inaugural address to the students of the Anderson College of Medicine, Glasgow, was delivered on October 10th by Professor J. Arthur Thomson of Aberdeen, who took for his subject "Some links between natural history and medicine." In olden days, he said, every medical practitioner was a bit of a naturalist, using plant simples and animal simples. The leech, his pocket companion for centuries, became synonymous with doctor. When from the animal prescriptions were subtracted those that seemed quite superstitious, there remained some which were reasonable and a few which suggested glimpses of modern discovery. Thus decoctions of ants, rich in formic acid, and diets of snails, the poor man's oysters, might well have useful results, and the use of snake's bile as a counteractive to the venom had been confirmed by modern research. There was no finer instance of the help that zoology could give to medicine than the contribution made by Dr. R. T. Leiper, a Glasgow graduate, to the life history of bilharzia. One of the characteristic features in the history of twentieth-century zoology and medicine had been the rise of protozoology into a science almost as important as bacteriology. Along with protozoology had developed medical entomology, for most of the formidable protozoa were carried and disseminated by insects. Greatly to the credit of zoology and medicine, working hand in hand, had been the elucidation of the part played by the mosquito in spreading malaria and of the tsetse fly in spreading sleeping sickness. It might be that a correlation of the facts of artificial parthenogenesis, the phenomena of regenerating lost parts, and the formation of gall growths, would throw some gleam of light on the dark problem of cancer. Again, there was great suggestiveness in the modern studies of senescence in animals and the various ways in which this was counteracted by processes of rejuvenescence. Zoology had been a trusty ally of medicine in the past, and the actual alliance was now firmer than ever.

Health and Housing in Dundee.

The Dundee census report was dealt with in our issue of August 26th (p. 400). The principal statistics for 1921, as given in his annual report by the medical officer of health, Dr. W. L. Burgess, include the following: The corrected death rate was 15.8, the infantile death rate 114 per 1,000 births, the birth rate 26.5. The death rate from pulmonary tuberculosis was 1, from all forms of tuberculosis 1.35, and from the principal epidemic diseases 1.09. The infant mortality was the lowest on record, as was also the tuberculosis death rate. There was very little epidemic disease; smallpox was entirely absent, and of enteric fever there were only seven cases. Dr. Burgess complains that notification of puerperal fever is not efficiently carried out by medical practitioners, partly, he observes, because the disease is not clearly defined. There were 193 notifications of ophthalmia neonatorum, being 1 in 23 births, against 1 in 19 births in the previous year; complete recovery took place in every case. Notification of tuberculosis is stated also to be very defective; of the 474 recorded cases, no less than 203 were detected by the tuberculosis officers, only 164 were notified by doctors, and 54 were brought to notice by the death returns. Dr. Burgess is doubtful whether any treatment should be carried out at the tuberculosis dispensary, and inclines to the view that its functions should be diagnosis, examination of contacts, supervision and education of patients, and the making of arrangements for institutional treatment, the work being supplemented in the homes of the patients. Child welfare work in Dundee suffered very seriously from the industrial depression, but considerable good was done in granting relief. Dr. Burgess holds, however, that, notwithstanding co-operation between the authorities interested, there was undoubtedly overlapping in this. Housing is discussed in relation to the society conditions of Dundee. A tremendous number of women are employed in industry, and have only an hour's interval to prepare and consume a meal, too often of tea and tinned food. Many of them are married and have to arrange for the care of their children, whom they may have to take to day nurseries. Also they dislike travelling any great distance to and from their homes in the dark of winter, while tram fares would add considerably to their outlays. In these circumstances their houses must be near their work, only a small rent can be paid, and small "tenement houses," as the term is used in Scotland, seem to Dr. Burgess to be properly permissible to meet the situation. Housing schemes have done much for the "middle" working class, but not for the "lower" working class areas, and it is stated that not a single family living in the slum areas has benefited by the new housing schemes.

Ireland.

At the annual election of officers of the Pharmaceutical Society of Ireland, held on Tuesday, October 10th, Dr. A. J. Barnes of Dublin was re-elected President for the ensuing year.

Royal Academy of Medicine in Ireland.

During the session 1921–22 the Academy consisted of 177 Fellows, 8 Members, and 20 Student Associates, numbers which are almost identical with those of the previous year. The accounts for the session have been audited, and the balance sheet shows the financial state of the Academy at the close of the session. The income during the year amounted to £441 2s. 5d., and the expenditure to £365 7s. 4d., thus leaving a balance to be carried forward to the next session of £75 15s. 1d. Professor Oskar Frankl again visited Dublin during the year, and he accepted the invitation of the Council to lecture before the Academy. Two lectures were given on April 25th and 27th, in the theatre of the Royal College of Surgeons, and both were well attended. During Professor Frankl's visit the Council heard of the hardships to which medical men in Vienna were subjected in consequence of the financial position there. So bad had things become that it was feared that owing to lack of funds the Medical Society of Vienna would have to close its library during the coming winter. The Council decided to open a subscription among the Fellows and Members of the Royal Academy of Medicine in Ireland; a sum of £45 10s. 6d. was collected and was sent as a donation to the library from the Academy. For this gift the sincere thanks of the Society have been sent by its President, Professor Eiselsberg, its Treasurer, and Professor Frankl.

A permanent contract has been entered into with Messrs. Cahill for the publication of the Academy's journal, and at a special meeting held on April 12th, 1922, the title of the journal was changed to *The Irish Journal of Medical Science*. Unfortunately, owing to the destruction of the premises of Messrs. Cahill by fire, the regular publication of the monthly numbers of the journal has been somewhat interfered with, but it is hoped that this difficulty will shortly be overcome. An important step has been taken by the establishment of an Obstetrical Research Committee in connexion with the Section of Obstetrics and Gynaecology. It is proposed that this Committee should prepare statistics and make collective investigations. Dr. Louis Cassidy, Master of the Coombe Hospital, Dublin, has been appointed secretary of the Committee, and the Council has voted a sum of 5 guineas towards the expenses.

The Council records with sincere sorrow the death during the session of two valued Fellows of the Academy, Sir Joseph Redmond and Professor Edward Taylor. Dr. Joseph Michael Redmond was one of the original Fellows of the Academy, and in 1907 and 1908, when President of the Royal College of Physicians of Ireland, he was President of the Section of Medicine. Several communications by him have appeared in the *Transactions*. Professor Edward Henry Taylor, who had been President of the Royal College of Surgeons in Ireland for just two years at the time of his death, was for many years an active member of the Section of Surgery, of which he was President when he died.

The Council is glad to be able to record a continued progress of the Academy. During the session twenty-five sectional meetings were held, at which the attendances were: Fellows 494, Members 30, Student Associates 164, and visitors 199.

Royal Victoria Hospital, Belfast.

The inaugural address was delivered at the opening of the winter session by the Marquess of Londonderry, the Minister for Education for Northern Ireland, in the Edward VII Memorial Hall of the hospital on October 12th. The Chairman of the medical staff, Professor Sinclair, F.R.C.S.Eng.,

said that Lord Londonderry had no need of any introduction to an audience in their province; he was indefatigable in visiting schools of all kinds, and in making himself directly acquainted in a practical manner with all forms of education. Perhaps there were one or two facts that he might mention to him, and he did so with a pride that was shared by all the members of the medical staff. Their medical students in all now numbered nearly 700, and this constituted them in respect of numbers the fourth largest school in the kingdom. Of the medical staff of the hospital, eight held professorships or lectureships in the university. Their board of management, under the chairmanship of Professor Lindsay, was enlightened, and was most liberal in meeting all their demands that tended to promote the welfare of the patients and the advancement of medical education. He assured Lord Londonderry of their gratitude and appreciation of his kindness in coming to open the session when there were so many calls on his time and energy. Lord Londonderry, who was received with the greatest heartiness by the students, expressed his pleasure in coming and thanked them for their welcome. He knew how much Lord and Lady Pirrie had done for their hospital and the value this hospital was to Ireland. He was especially interested in the clinical teaching, as it was part of the university work. It was his desire to widen the present avenues to the university and to open up more; the drawback in the past was the lack of opportunities for everyone to equip themselves intellectually for the battle of life. His primary care was for the young: his audience were of an age to know their responsibilities; those for whom he worked in primary and secondary schools were not of an age to do so; and he hoped that in time to come, as their education system became more and more perfect, he might be allowed to address on some future occasion another audience in that chamber even better equipped for their advanced studies that now occupied them. He urged them and all students to fit themselves for and to interest themselves in all that was passing around; it was a duty to themselves and to their province and to their Empire. Dr. McKisack proposed and Mr. Mitchell seconded a most hearty vote of thanks to Lord Londonderry for his enlightened and stimulating address, and this was conveyed by the chairman.

VITAL STATISTICS OF NORTHERN IRELAND.

The first quarterly return of marriages, births, and deaths issued by the Government of Northern Ireland has been published.[1] The population of Northern Ireland in the middle of 1922 was estimated to be 1,284,000, inclusive of military. The number of marriages registered during the fourth quarter of 1921 was 1,855, representing an annual rate of 5.8 per 1,000 of population. During the quarter ending March 31st, 1922, 7,639 births were registered in Northern Ireland, equal to an annual birth rate of 23.8 per 1,000 of the estimated population; in England and Wales the corresponding birth rate during the quarter was 22.2. The deaths registered during the first quarter of the present year numbered 6,422, representing an annual death rate of 20 per 1,000; the corresponding mortality in England and Wales was 17.7. In the county boroughs and urban districts, which contained nearly one-half of the total population, the equivalent annual death rate for the quarter under review was 28.8 per 1,000, and the birth rate for the remainder of Northern Ireland was 20.7. The death rate in the county boroughs and urban districts was 22.1 per 1,000, and the corresponding rate for the remainder of Northern Ireland was 19.2. The urban mortality from the principal epidemic diseases recorded during the quarter was equivalent to a rate of 1 per 1,000, the death rate from these diseases for the remainder of Northern Ireland being 0.6.

Mauritius.

THE CASE FOR SANITARY REFORM.

NEARLY two years ago the Secretary of State for the Colonies commissioned Dr. Andrew Balfour, C.B., C.M.G., to visit the island of Mauritius for the purpose of investigating its sanitary condition. The Report[2] has now been issued, and discloses the extent and urgency of the reforms which should be instituted. The island has an area of fully 700 square miles and a population of 376,000, of whom about 10,000 are whites, mostly French. The working classes, who are in great proportion descendants of liberated slaves, are stated to be highly intelligent. The population, however, is very heterogeneous. The density of population is over 500 per square mile, and the death rate is from 32 to 35 per 1,000, but in 1919 an influenza epidemic raised it to close on 65 per 1,000. In 1920 the rates in different districts ranged from 23 to over 40 per 1,000. Dr. Balfour devotes the earlier part of his report to communicable diseases. Dr. J. F. Kendrick had made, on behalf of the Rockefeller Foundation, and not very long before Dr. Balfour's visit, a survey of the island in relation to ankylostomiasis, and estimated that about 226,000 persons—three-fifths of the population—were infected. For prevention of infection of influenza amongst contacts value was attached to a measure advocated by Dr. J. A. Taylor of the Uganda Medical Service. It consisted in the application every three hours on the tongue of contacts of two or three drops of a mixture of equal parts of ordinary tincture of iodine (*B.P.*) and native honey. The accommodation for leprosy Dr. Balfour condemns unsparingly. The single asylum, he says, is "a disgrace to the colony." Against malaria much good work has been done in giving effect to the recommendations of Sir Ronald Ross, who had visited the colony a number of years previously. Malaria, however, still ranks first as a destroyer of life in Mauritius. Tuberculosis comes next, being spread by faulty housing, poor food, unsuitable clothing, poverty, alcoholism, and predisposed to by ankylostomiasis. As regards alcoholism, a commission, appointed by Sir Hesketh Bell in 1919, reported on the paramount importance of combating the increasing peril. Conflicting interests, however, are powerful. The State derives large revenue from the liquor traffic; the distillers sell their rum to the trade; the traders retail it to the public; and the sugar planters supply the distillers with the molasses. The retailers are mostly Chinamen. The commission did not recommend immediate prohibition, but rigorous control, and the manufacture of a light drink, such as Kaffir beer. Dr. Balfour hopes also that what is known as the Durban scheme will be adopted, its main feature being the vesting in the Town Council of a monopoly for the entire control of the manufacture and sale of the native beer, all profits going to a special "Native Administration Fund" for the general well-being.

Under the heading "Medical facilities" there is consideration of hospitals, dispensaries, infirmaries, orphanages, crèches, maternity and child welfare, and medical inspection of schools. "Sanitary matters" are next reported on—the medical and inspectorial staff, water supply, collection and disposal of excreta and general refuse, solid and liquid, slaughterhouses, meat markets, bakehouses, milk supply, washing and disinfection, offensive trades, housing and town planning, schools, prisons, quarantine, and so forth. In short, nothing relevant appears to have been overlooked by Dr. Balfour, and he amply demonstrates that in practically every direction there are many defects. He makes a series of recommendations for reorganization of the medical and health department of the colony. An important step already taken is the appointment of a competent consulting sanitary engineer, who had reached the island before Dr. Balfour left. The scheme has obviously been thought out with care and knowledge, and though the total salaries suggested amount to fully £30,000 instead of £10,000 as at present, it is urged that they relate really to emergency conditions. In addition qualified sanitary inspectors are required, district mosquito brigades, improved equipment and increase of the staff of the bacteriological laboratory. In short, the question is fundamentally financial. Present-day knowledge of disease prevention is capable of being effectively applied to the numerous difficulties that have to be overcome. The existing conditions are appalling, and are forcibly described and photographically depicted by Dr. Balfour. A general improvement and development fund of ten million rupees had already been voted by the Council of Government, and an important part of the Commissioner's duty was to advise as to the spending of it. The task would have been easier if the sum had been larger, but the fund as invested yields Rs.750,000 a year. Of the measures needed for sanitary improvement thirteen are bracketed as equal in importance; they include general antimalarial measures, sanitation of Port Louis, various improvements in sewerage and scavenging, abolition of certain slum areas, improvements in milk supply and water supply, bacteriological laboratory establishment and

[1] Belfast: Published by His Majesty's Stationery Office. Price 1s. net.
[2] Report on Medical and Sanitary Matters in Mauritius, 1921. Waterlow and Sons, Limited, London Wall. 1922. (Fcap., pp. 167, with numerous photographs.)

extension, and microscopes for hospitals and dispensaries. In respect of soil sanitation, there is to be co-operation between the Mauritian Government and the Rockefeller Foundation. About the fundamental economy of the whole proposal there can be no doubt. Dr. Balfour writes regarding Mauritius: "It is a marvellously fertile island, and the climate for the greater part of the year is delightful; but it has drifted into a state which is inimical both to efficiency and industry, and has, with good reason, gained a reputation which repels those who might otherwise visit it, either for purposes of commerce or pleasure." The report shows how that bad reputation may be mended, and it is for the authorities to give serious heed to the advice which they have received from their Commissioner.

Correspondence.

THE DIFFERENTIAL DIAGNOSIS OF SMALL-POX AND CHICKEN-POX.

SIR,—At the season favourable for the spread of small-pox, of which 21 recent cases in and near London have yielded 7 deaths (several at first were taken for chicken-pox), it may be of service to utter a reminder and to mention that the diagnostic criteria which have proved in practice to be the most reliable in differentiating some 500 cases of chicken-pox, mostly adults, from among about 11,000 cases of small-pox and similar diseases in London during the last twenty years are four in number—namely:

1. The presence or absence of marked general prostration in the early stages.
2. The relative density of the eruption on irritated surfaces.
3. The relative depth of the lesions in the skin.
4. The progressive maturation of the lesions.

As any acute febrile attack, however mild, may in reality be small-pox, every such illness calls for an examination of the skin and buccal mucosa for haemorrhages, papules, pocks, or other blemish, in order to prevent the escape of a case which, however trivial in itself, may yet occasion many fatalities.—I am, etc.,

London, W., Oct. 16th. W. McC. WANKLYN.

PSYCHO-ANALYSIS.

SIR,—As Silas Wegg "dropped into poetry" only on being pressed, so unwillingly I feel I must "drop into print," for if Dr. McBride's letter in your issue of October 14th does not receive some reply the silence might be construed as consent, which is by no means the case.

Your correspondent desires that psycho-analysts should give a "reasoned defence" of their position, and presumably this should take the form of a letter to you.

The thing is impossible. As well ask a disciple of Darwin to defend his position in a letter.

The word "psycho-analysis" connotes three distinct subjects—namely, (*a*) a method of mental examination, (*b*) a theory, and (*c*) a method of treatment. Which of these is to be defended? Let us concentrate for a moment on (*b*)—the theory. The theory of Freud deals with normal mind, abnormal mind, dreams, wit, folklore, ethnology, myths, aesthetics, the science of religion, etc. Once again I ask, Is the relationship of the theory to all these to be defended? Or, if in one instance only, which one?

A reasonable exposition on any one of these matters is impossible in a letter, but in book form there is no lack of detailed description. It would be an easy task to give a list of books on psycho-analysis. I would recommend, however, to anyone interested in Freud's theories to begin with Hart's *Psychology of Insanity*. The student should then read *The Interpretation of Dreams* by Freud. Afterwards he has many books to choose from, but he must not omit to read Freud's *Introductory Lectures on Psycho-analysis*, just published. If, having completed these studies, he disagrees with their teaching, let him give his reasoned argument.

As things are at present, what one often meets with is ignorant prejudice. Some time ago a man whose identity was at the moment unknown to me, as mine was to him, said to me that he was going to make a speech on psycho-analysis. I asked him if he knew anything about the subject, and he was obliged to confess ignorance. I do not know if the speech was delivered, but, if so, doubtless it was received with prolonged and great applause; this state of affairs is not rare. Assertion takes the place of argument, and if this is all that is required I can also assert. I have no hesitation in saying that the theory of psycho-analysis throws a flood of light upon numerous cases of mental disturbance, not only in the severe types commonly called insanity, but in the so-called border-line states, numerous examples of which I see at my out-patient department at King's College Hospital. I would further assert that a large and increasing number of my colleagues agree with what I say.

Now, if any anti-Freudian who has seriously studied the matter will give us a book containing the results of his considered judgement, I can guarantee it will be received with sympathy and read with care, nay, even with avidity. Personally I do not like the Freudian theories. They are hurtful to my self-esteem and they offend my aesthetic sense, and if anyone can prove that they are wrong these emotional disturbances will cease in me. But the question is not what one likes, but what is true.

I hope Dr. McBride does not think I include him in the company of those who have not studied the matter. He knows best. I would, however, say to him and to Sir Clifford Allbutt, to whom he refers, a modification of the words of Gamaliel:

"For if this counsel or this work be not of the truth it will come to naught, but if it be the truth ye cannot overthrow it, lest haply ye be found even to fight against the truth."

—I am, etc.,

R. H. STEEN, M.D., F.R.C.P.

City of London Mental Hospital, Oct. 16th.

SIR,—It is not without provocation that I write, for I have been reading the report of the proceedings at Glasgow, particularly in regard to the discussions of "psychotherapy." Where I got facts derived from experience I was content, but all too frequently I had the impression of being plunged into that style of high-sounding jargon devoid of any scientific thought that reminded me of the doctors of Molière.

I propose, however, to deal with these papers not by criticizing them point by point, but by taking the subject at other bearings so as to show the invalidity of the foundation on which this entire superstructure rests.

Psycho-analysts have brought themselves to a point at which they depend on Freud as the old schoolmen acknowledged the authority of Aristotle precisely in those parts of his philosophy where the great Stagirite was weakest. Galileo disposed of that worship by his experiment of dropping his two pellets from the leaning tower of Pisa, and by putting mechanics on a sane basis he prepared the scientific basis of our modern world. I have proposed to render a similar service to psychology,

Suppose that we were able to formulate what, by an analogy from chemistry, we might term the elements of thought, and that, having given such elements, we could show that by the syntheses of these all forms of psychic movement, the simplest and the most complex, could be exhibited, would we not have the master key to all psychology? It leaps to the eyes that it would be so. But that is precisely the problem which I set before myself and wrestled with for twenty years, until, at length, I was able to formulate these elements, or, as I have called them, the "fundamental processes of the mind."*

Space does not permit me to enter into detail as to these fundamental processes, but one of them is association. That is not, of course, the only fundamental process, for, otherwise, no mechanism of mental activity could be demonstrated, whereas the manner of functioning of these processes corresponds to the working of the brain, which may be taken as the living diagrammatic form of the whole complex of the movements.

Now I wonder how many, even of ardent Freudians, have gone to the original authority to examine their master by such a test as here indicated. Those who do will not find a Galileo; they will meet with the method and manner of the schoolmen.

Freud takes association as the master key, and with this assumption, so absurdly inadequate in view of the searching analysis and the rigorous proof demanded by science, he builds up his system. I say nothing at present as to his great sex idea, his exposition of memory and of dreams, except to remark that I will later bring to demonstration that

* The exposition of these fundamental processes forms the backbone of *Psychology: A New System*, in which, by methods of rigour such as, I venture to say, had never before been introduced into these studies, I showed that these fundamental processes were both necessary and sufficient to cover the whole field of thought. This book has been sold out, but another and better exposition, *Principles of Psychology*, will appear in January.

his reasoning in these questions is unscientific and that certain of his conclusions are ridiculous. I wish to concentrate on that point of his meagre offering of analysis, for, to put an analogy, if a man explained the steam engine by saying that nothing had to be considered but latent heat, he would not only have talked nonsense about the steam engine, but he would have given a false notion of latent heat.

Freud has given us a false theory of mental process, and on his bright particular star of association he has talked moonshine.—I am, etc.,

London, N.W., Oct. 16th. ARTHUR LYNCH.

SIR,—I wish to comment on one of the numerous misconceptions to which Sir Clifford Allbutt has subscribed in his latest attack on psycho-analysis (in an address which Dr. McBride finds "admirable, scholarly, and scientific"). He identifies the practice of psycho analysis with that of the confessional (as does also Dr. McBride in his letter), and states that it has been known for centuries in the (Catholic) Church.

Now the most elementary acquaintance with psycho-analysis will inform one that it is concerned with processes of which the subject is entirely unaware, with what is called the unconscious mind, so that, as priests obviously deal only with conscious processes, they are concerned with a completely different sphere. This fact alone, apart from other fundamental differences, would serve to distinguish the practice of psycho-analysis from that of the confessional; in fact I know of no feature common to the two beyond the professional secrecy enjoined in both.—I am, etc.,

DOUGLAS BRYAN,
Honorary Secretary of the
London, W., Oct. 16th. British Psycho-Analytical Society.

ANIMAL EXPERIMENTS.

SIR,—I was not a little amused by your editorial note on "Animal Experiments in 1921" in your issue for August 26th, 1922.

The law under which these experiments were done I presume is the law advocated by the antivivisectionists, such as Mr. Coleridge and Dr. Hadwen. The reason for my amusement is the statement that "the law demands that the operation shall be performed *antiseptically so that the healing of wounds shall, as far as possible, take place without pain.*" (Italics my own.)

Now Messrs. Coleridge, Hadwen, and their friends deny the germ theory *in toto* and also deny the value of antiseptics. They ridicule all such precautions.

What a tribute their law pays to the value of antiseptics as a means of preventing pain and facilitating healing! I admire their unconscious advocacy of antisepsis and wonder what has become of their consistency.—I am, etc.,

W. W. KEEN,
Emeritus Professor of Surgery, Jefferson Medical College, Philadelphia.
1520, Spruce Street, Philadelphia, Sept. 25th.

THE TREATMENT OF ECLAMPSIA.

SIR,—I was sorry I was unable to be present at and to take part in the discussion at Glasgow on the above as I wished, for I must confess it would have pleased me to learn that all is not well with the toxaemia hypothesis of eclampsia. In March of this year in your columns I read that "the toxaemic origin of eclampsia is now accepted." In your issue of to-day we learn that this hypothesis "is not entirely free from difficulties." This, I think, is very satisfactory, because, as I have endeavoured to show, the toxaemic hypothesis is untenable.

Pathologists have already spent such an enormous amount of time in the vain search for a specific toxin explaining eclampsia that it is difficult to believe they are on the right track, and not surprising to find it urged that those who have to treat eclampsia can wait no longer for them. There are, however, already several indications before us which indicate the nature and the cause of eclampsia—that it is simply a uraemia produced by the condition of pregnancy. First, I would put the treatment which experience so far has shown to yield the best results; this treatment, plainly, is the treatment for uraemia. Next, I would remind you of the incidence of eclampsia; this is so peculiar that it shows that the disease cannot be due to any peculiarity in the contents of the uterus. And in itself, it indicates very strongly that the pre-eclamptic state and eclampsia are due to a faulty metabolism induced by pressure rises in the abdomen. This is supported by the discovery that conditions which diminish the pressure in the abdomen are of very great assistance in dealing with the disease. The Dublin school urges placing the eclamptic patient in the lateral position, otherwise, they say, she will drown in her own secretions. The importance of the accumulation of mucus in the bronchi and smaller channels is obvious; but it is doubtful whether all this comes from the nose and mouth. By placing the patient in the lateral position the pressure in the abdomen is much reduced; not only does this afford the best possible condition for the re-establishment of blood flow through the capillaries of affected viscera, but inquiry may possibly show that it so alters the pulmonary circulation as to check or limit pulmonary congestion and oedema. The puncturing of the membranes, allowing escape of liquor amnii, in order to decrease the volume of the uterus was mentioned by Dr. Campbell, and by earlier writers—for example, McClintock—was noticed as a measure which sometimes stops the fits. Such a diminution in uterine volume can only have one effect, and that is to reduce the intra-abdominal pressure. Conversely, we see how it is that in concealed accidental haemorrhage (in which the uterus becomes very large and tense) eclampsia rapidly ensues, and that the ordinary treatment of tightly binding the abdomen is about as bad as it possibly can be. It cannot stop the bleeding; it simply makes the circulatory conditions under which the liver and kidneys labour worse—if the pressure caused by the distending uterus has not clamped their capillaries the treatment does so.

The emptying of the uterus is a very vexed question. The treatment advised is not to interfere unless absolutely necessary; but all the world knows that the termination of the pregnancy is the best possible thing, if it can be done safely and without making the patient worse. It seems to me that it is the participation of the patient in the labour which explains the anomaly. If a patient acutely ill with the pre-eclamptic process has induction of labour practised or accouchement forcé, the straining which necessarily follows and all her movements facilitate the morbid process, and hence we get bad results. That induction should give better results than accouchement forcé is intelligible; in the former the reaction develops slowly; the latter is a much more rapid affair. In this respect the effects of labour must be remembered; in the pre-eclamptic state the activities of (natural) labour are largely responsible for the institution of fits. Such an objection to Caesarean section cannot, of course, apply, because the patient is anaesthetized. But here there are two distinct states to consider. There is the state of the patient who is desperately ill, who already has had many fits, and whose liver and kidneys are very probably already the seat of extensive necrosis; any treatment under such conditions cannot possibly hope to save the patient. But there are cases in which the patient is not so ill, who has had less fits, and whose liver and kidneys presumably are not so diseased, are not perhaps at the moment necrosed, being simply rendered inactive, and who with the relief of pressure may possibly recover. We cannot compare the results of Caesarean section in these two types of case; nor should we condemn the operation because of bad results in the first class of case.

Many successes after Caesarean section for eclampsia have been recorded, and it is plain from the comments reported in your issue that several writers regard the operation favourably for eclampsia. The point is, the operation should not be withheld as a last resource; as a measure for eclampsia it should be employed, it seems to me, not just to deliver the woman but to prevent any possible straining at a period of the morbid process when any increase of pressure within the abdomen may just be the turning point from a possible recovery to an absolutely certain death. If a woman has had one or two fits and becomes conscious between them, as patients often do, she is bound to strain during delivery by ordinary channels unless deeply under an anaesthetic.—I am, etc.,

London, W., Oct. 14th R. H. PARAMORE, F.R.C.S.Eng.

UNSUSPECTED PREGNANCY.

SIR,—If the confidence of your correspondent in his *ipse dixit* as to the impossibility of a pregnant woman of average size, etc., not suspecting her condition remains unshaken, I wonder if it will survive the shock of reading the following true tale.

When driving past a farmhouse some years ago I was hailed by the farmer's son, who told me that his aunt, who

had come to keep house while his mother was away on holiday, had been taken ill. I found the aunt, a woman 46 years of age, of "average size," sitting in the kitchen and complaining of abdominal pain. As the pain seemed suggestive of labour pains, I asked her if she was pregnant. She said, No, and seemed surprised at the question. I advised her to go to bed. On examination I found the head low down, and about an hour later a full-time male child of "average size" was born, followed shortly by another child, mummified, and apparently of about 7 months' development.

The shy fiancée of the farmer's son, the only other woman about, was my "nurse," and her plight, as affected by the surprising event, obsessed as she was by the delicacy of her own position, was a curious blend of the comic and the pathetic. The mother was a married woman, and had had one child previously, a girl 18 years of age at that time. Husband and wife were delighted at the birth of a son, and had had no idea of the existence of pregnancy, twin or other. "Wind" and "change of life" had been their diagnosis, and they had been supported in this by their medical adviser, whose name I do not think for a moment was Dr. Graham Grant. *Verb. sap.*—I am, etc.,

Llansantffraid, Mont., Oct. 14th. W. H. Lewis.

THE RADICAL PREVENTION OF VENEREAL DISEASE.

Sir,—I thought the published reports of the National Council and the Ministry of Health reached limits of insipid irrationality and smug futility; but Mr. Herbert Caiger's letter (British Medical Journal, October 7th, pp. 661–2) soars to even dizzier limits. His solution of the venereal problem is to "circumcise the heart" and inhibit the sexual instinct.

But Mr. Caiger must not be permitted to discredit the fact that disinfectants do disinfect. He can easily prove of course that soldiers issued with little bottles of lotion without instruction got gonorrhoea even though they drank the lotion; that other soldiers who used their calomel ointment as shaving cream got chancres on their genital organs notwithstanding; and that soldiers who threw away their outfits deliberately nevertheless acquired self-inflicted venereal disease (to avoid the firing line); and even that soldiers to whom no outfits were issued were not protected by the disinfectants they did not get; but what Mr. Caiger cannot disprove is:

1. That without an issue of disinfectants the New Zealand Division had in 1917 an average of 1,000 venereal disease soldier patients.
2. That when in 1918 the issue of disinfectants was made fairly efficient (it was never complete) the New Zealand Division had less than 500 venereal disease soldiers on the average (a saving of about 4,000 or 5,000 soldiers for the year 1918 alone).
3. That when in 1919, through religious and political interference, the issue of disinfectants and the prophylactic lectures were stopped, the New Zealand Division again had to maintain over 1,000 venereal disease soldier patients.

As to his question, "Will the failures be less in civil life?" Of course they will. In civil life venereal disease is never a benefit, and always a loss of time and money. For the civilian the choice often is: Keep clean and keep your job. For the soldier: Keep clean—and be shot!—I am, etc.,

London, W., Oct. 12th. Ettie A. Rout.

CARDIOLOGY.

We have received from our reviewer the following note on Dr. Harrington Sainsbury's letter published in the Journal last week, p. 703:

"It is not denied that changes in pressure within the right auricle and contiguous veins are associated with, and cause, variations in volume. It does not follow, however, that tracings which record changes in volume are necessarily correct graphs of pressure. The blood pressure in the carotid artery (the artery covered by the receiver) is many times greater than that in the jugular, yet the movement of the recording lever, wave '*c*,' produced by the arterial pulse, may be insignificant in comparison with that caused by the jugular pulse. In discussing the wave '*c*' Dr. Sainsbury stresses the point that 'the force which generates it is by far the most powerful of the forces operating at the root of the neck and appearing as pulsations. Its waves therefore *should be* the most pronounced of the three.' They should, if the polygraph recorded the forces which produced pulsations or the degree of pressure in the vessels covered by the receiver; but it does not."

Obituary.

EDWARD COLLETT HORT, B.Sc.Paris, F.R.C.P.Edin.

With the premature death of Dr. E. C. Hort on the morning of October 15th at Cannes, after a long and trying illness, a man of exceptional ability and charming personality has passed beyond the veil. He was remarkable for originality, not without a touch of genius, for his independence, and for the amount of work that he accomplished within a comparatively short period.

Edward Collett Hort was born in 1868, the third son of the Rev. F. J. A. Hort (1828–92), Hulsean Professor of Divinity in the University of Cambridge, who was described as the "greatest English theologian of the nineteenth century." His elder brother, Sir Arthur Hort, sixth baronet, was until lately an assistant master at Harrow; a younger brother is a member of the profession. Educated at Sherborne and at Emmanuel (his father's) College, Cambridge, Edward Hort finished his medical education at Guy's Hospital, obtaining a medical qualification (L.S.A.) in 1896 and marrying in the same year. The next ten years or so were spent in various parts of the world, some in America, where he had many adventures, but eventually he settled down to ordinary medical work in the south of France. About 1907 he came back to England and bought a practice at Torquay; his outlook on life now appears to have undergone a change, for he became a keen laboratory worker. Hort, indeed, was one of those who develop late in life, being nearly 40 when he started on the work which he henceforth pursued with ceaseless, almost feverish, energy in spite of the handicaps of bad health and the necessity of supporting his family by general practice. While at Torquay observation of a patient with a haemorrhagic form of disease attracted his attention to the use of horse serum and led to the appearance of his first published papers—namely, on its use in gastric and duodenal haemorrhage (1907, 1908), based on the hypothesis that the ulceration was the outcome of autolysis due to proteolytic bodies in the blood, and that this process could be counteracted by the administration of horse serum. After about a year he migrated to Harley Street, and at once became busily engaged in a high-class practice. For a time he was assistant physician to the Italian Hospital, but his mind was now actively evolving problems in connexion with bacteriology and serology, and every spare moment was devoted to research work, which was carried out first in Sir Lenthal Cheatle's private laboratory at King's College and then at the Lister Institute. A grateful patient supplied an endowment, and this Constance Trotter Fund enabled him to enlist the help of skilled collaborators. A constant stream of papers now began to appear—for example, in the *Journal of Hygiene* (several in collaboration with Dr. W. J. Penfold), the *Quarterly Journal of Medicine* (1911), the *Proceedings of the Royal Society* (1910, 1912), the *Journal of Vaccine Therapy*, the *Proceedings of the Royal Society of Medicine*, the *Journal of the Royal Army Medical Corps*, the *Lancet*, and our columns—on various aspects of bacteriology, such as micro-organisms and their relation to fever, experimental fever, salvarsan fever, vaccines and fever, autotoxaemia and infection, and the antitryptic index.

Early in 1914 he investigated the bacteriology of typhus fever with Dr. W. W. Ingram and described a filter-passing organism, infective to monkeys, which was pleomorphic, presenting a cocco-bacillary form as a harmless phase. This was the first of a series of papers which came out during the war, advocating the recognition of pleomorphism or a complex life cycle for bacteria. Among these were four on cerebro-spinal fever, embodying the results of work done while honorary physician and director of the bacteriological staff of the Addington Park War Hospital (1914–18) in collaboration with Drs. C. E. Lakin and T. H. C. Benians and later with Captain A. H. Caulfeild, C.A.M.C. He contended that the meningococcus is not the cause of the disease but merely a late non-infective phase in the life history of an organism allied to the Ascomycetes and infective only in its earlier phases; this conclusion involved him in a controversy with Mr. J. Ramsbottom, mycologist to the British Museum, and his advocacy of the complex life cycle of the bacteria did not gain any acceptance, and indeed very little notice, from other bacteriologists except for a generous plea for recognition from Professor J. G. Adami (*British Medical Journal*, 1916, ii, 525). Whether or not Hort's revolutionary conception may eventually prove to be correct, there is no doubt of the enormous care that he devoted to avoid the

obvious fallacies of contamination, and that his enthusiasm and ingenuity command admiration. His paper, entitled "The meningococcus of Weichselbaum," appeared in the *British Medical Journal*, 1917, ii, 377, and was illustrated with drawings to show his conception of the development of "giant meningococcus."

While at the Addington Park War Hospital he contracted dysentery and was extremely ill; from this he slowly recovered and, after serving as temporary lieutenant-colonel in charge of a convalescent hospital on the Riviera, turned his energies into another direction, the invention of suitable foods for the starving Russians, as an interlude to the bacteriological work which it was his ambition to see overwhelmingly proved and accepted. But this was not to be; last year symptoms of visceral tuberculosis became urgent and the disease slowly gained on him.

With his most active and energetic nature it was not surprising that Hort had other interests besides bacteriology and that he was a skilled musician and a keen fisherman. His special charm of manner and lovable disposition brought him friends wherever he went. He leaves a devoted wife and two daughters, the elder of whom had assisted him in his work.

We are indebted to Dr. J. G. Adami, F.R.S., Vice-Chancellor of the University of Liverpool, for the following appreciation of Hort and his work in bacteriology:

It is as easy to criticize and damn with faint praise men of the type of my old friend E. C. Hort as it is difficult to give a just appreciation of their sterling virtues. For in truth he was a genius, and geniuses are difficult to live with, not easy to guide, and have a way of "kicking over the traces" that is disconcerting to ordinary mortals. Particularly is this so when they have Irish blood in their veins. What is more, he was an example of the man who develops late and in an unacademic manner. I knew him as an undergraduate at Cambridge in the late eighties or early nineties—frank, cheery, and irresponsible. The contrast between him and his most studious father, the scholarly colleague of Professor Westcott, was most striking. The last accusation one would have made was that he was either given to hard work or scholarly. And the air of boyish irresponsibility clung to him throughout life. Nor so far as I can learn was his career as a medical student in any way distinguished. If I am not mistaken, the death of his father rendered it necessary that he should qualify without delay. Leaving Cambridge he entered at Guy's and obtained the L.S.A. in 1896. I lost touch with him for some years until from 1908 onwards there came pouring out a series of papers upon serology and immunization that showed the presence among us of a man of pronounced originality, an observer of phenomena passed over by most of us. Who, for example, but Hort would have investigated the pyrogenic effects of (presumably) pure distilled water? All of us who had been interested in the bacteriology of cerebrospinal fever had observed irregular forms of the meningococcus: we, however, had focussed our attention upon the detection of the normal forms: Hort by preference concerned himself upon the abnormal and interested himself in them as throwing light upon the life-cycle of the organism.

What had happened? Merely this, that engaged in ordinary practice he had become interested in immunology—so interested that notwithstanding the very partial training he had received in bacteriological technique he determined to test the phenomena for himself. Gifted with a natural love for experiment, and with a manual dexterity much above the ordinary, he established a laboratory in his own house, and with little help from outside, in the shape of routine training in recognized laboratories, he worked at problem after problem. Doubtless he learned much from the well-trained co-workers when in the latter years he attached them to himself. But working incessantly and with intense enthusiasm he elaborated a technique that was remarkable. I shall not easily forget the revelation of methods, as precise as they were original, that he gave in 1918 when a committee appointed by Sir Alfred Keogh, and consisting of Sir William Leishman, Colonel Harvey, and myself, visited his laboratory in order that he might demonstrate to us his work upon the pleomorphism of the typhoid bacillus which he had reported to the War Office. We might differ from him as to the interpretation to be given to his facts, but the facts were there before our eyes—the growth and multiplication of the typhoid bacilli at a certain stage and under certain conditions by processes other than those of simple binary division.

In short, as one who was not a product of the schools, he saw bacteriological phenomena from an independent angle, and as a consequence he obtained novel results of the first importance. It is quite natural that men of his type are misunderstood, that the ordinary bacteriologist brought up to employ the routine methods should mistrust the findings of one who has no credentials of work performed under accepted leaders in well known laboratories. It is true also that as a self-taught man his training was imperfect; with deep knowledge of some parts of his subject he was poorly grounded in collateral branches of science. His knowledge of protistology, for example, was incomplete. Nor as an independent worker did he possess that body of colleagues in different branches of science to whom to turn for advice and assistance in the explanation of his facts. It is the results obtained, and not necessarily his interpretation of them, that in my opinion must be accepted and must revolutionize our views as to the life-history and properties of the bacteria. I am convinced that future years will establish Hort as one of the masters in bacteriology.

Dr. Pratul Kumar Ghosh, of Treeton, near Sheffield, who died on October 1st at the age of 41 years, was the son of a well-known Hindu medical practitioner of Calcutta and Dacca. Educated at Dacca College and the Universities of Calcutta and Edinburgh, at the beginning of the war Dr. Ghosh joined the Indian Ambulance Corps, and served for nine months at the York Place General Hospital, Brighton. Returning to Edinburgh he graduated M.B., Ch.B. in 1915, and, after acting as an assistant, in 1919 he acquired a practice at Treeton and Woodhouse, near Sheffield. Here Dr. Ghosh soon endeared himself to his patients by his devotion to their interests and at the same time gained the goodwill of all his fellow practitioners in the neighbourhood by his loyalty to professional ideals. His death from pneumonia on the eve of a well-earned holiday was the result of several years of overwork in a heavy industrial practice. Dr. Ghosh was a member of the Rotherham Division of the British Medical Association, and was also a popular member of the Renishaw Park Golf Club. He was surgeon to the Rothervale collieries and acted as one of the medical referees for the Yorkshire Coal Owners Indemnity Company and several life assurance companies. His remains were cremated at Sheffield on October 3rd. He leaves a widow and one child, with whom all classes in the district deeply sympathize.

Universities and Colleges.

UNIVERSITY OF CAMBRIDGE.

At a congregation held on October 13th it was resolved to confer upon Professor H. R. Dean, D.M.Oxon., newly appointed to the chair of pathology, the complete degree of M.A. *honoris causâ*. Professor Dean has since been elected to a professorial fellowship at Trinity Hall.

The following medical degrees were conferred:

M.D.—F. G. Lescher.
M.B.—H. L. Willey.
M.B., B.Ch.—C. S. Atkin, A. E. Roche.

The following have been approved in Part II (Pharmacology and General Pathology) of the Second M.B. Examination, October, 1922:

B. E. Ahrens, E. L. Auguste, G. W. Bamber, C. M. Barker, W. A. Barnes, C. O. Beane, I. C. P. Beauchamp, S. A. Belshaw, W. A. Bourne, C. W. Brook, G. H. Brown, C. B. Buckley, R. C. L'E. Burges, R. W. Butler, A. Carnegie Brown, A. N. Carruthers, W. E. Chieseman, J. E. Church, W. R. F. Collis, C. K. Colwill, W. F. Cooper, W. S. C. Copeman, R. W. Cuningham, R. Cunningham, T. S. Dewey, M. S. Dephurst, J. Dockray, C. E. Donaldson, R. W. B. Ellis, T. I. Evans, T. F. Everett, E. O. Fox, J. H. Gibbens, H. K. Goadby, F. Goldby, E. P. Gough, B. Graves, R. A. P. Gray, E. J. Greenwood, H. F. Griffiths, J. L. Groom, C. J. Grosch, K. H. A. Gross, W. S. Grove, R. F. Guymer, H. J. Heathcote, A. C. de B. Helme, J. D. Hindley-Smith, J. C. Hogg, A. T. Howell, J. T. Irving, D. McI. Johnson, W. E. Joseph, J. J. Keevil, L. A. Key, M. E. Lampard, W. E. B. Lloyd, T. F. McNair Scott, J. P. Marsden, F. Martyn, R. D. Milford, J. G. Milner, V. S. Mitcheson, E. D. Moir, E. G. Morris, J. D. R. Murray, H. P. Nelson, C. E. Newman, H. W. Nicholson, G. F. Oakden, C. L. Owen, L. J. Panting, R. W. L. Pearson, H. C. Pierce, C. G. E. Plumstead, J. D. Procter, P. E. Pym, J. H. Randall, A. M. Rhydderch, J. R. W. Roberton, R. P. Ross, G. L. F. Rowell, T. W. E. Rowden, H. W. Sadlier, K. G. Salmon, H. B. Savage, S. W. Savage, H. Scudamore, E. M. Shackel, R. E. Smith, W. Smith, W. E. Snell, H. B. Stallard, G. M. Tanner, B. C. Tate, G. K. Taylor, E. J. E. Topham, E. A. Trim, C. I. Tuckett, J. G. E. Vachell, J. L. Warner, C. F. Watts, D. B. Wilson, H. G. Wimbush, R. M. Windeyer, L. B. Winter, J. T. Woodhead, E. C. Wynne-Edwards.

The plan for the December examinations for medical and surgical degrees has now been published. The first M.B. examination and Part I of the second M.B. examination will begin on December 11th, Part I of the third M.B. on December 12th, and Part II of the third

M.B. on December 13th. The M.Ch. examination will begin on December 12th.

The Gedge prize for original observations in physiology has been awarded to F. J. W. Roughton.

VICTORIA UNIVERSITY OF MANCHESTER.

DR. K. C. MIDDLEMISS has been appointed Lecturer in Infectious Diseases.

Professor R. B. Wild has been appointed as representative of the University upon the General Medical Council, and Professor W. W. C. Topley as the representative upon the Council of the Lister Institute of Preventive Medicine.

UNIVERSITY OF SHEFFIELD.

DR. ROBERT PLATT has been appointed Demonstrator in Pathology and Bacteriology.

UNIVERSITY OF WALES.

THE following candidate has satisfied the examiners in Medicine, Surgery (with distinction), and Obstetrics and Gynaecology: Eroica Rowe Lewis.

UNIVERSITY OF GLASGOW.

THE following degrees were conferred on October 16th:

M.D.—*A. Scott, †W. M'Kendrick, D. Cameron.
CH.M.—S. Mort.
D.SC.—W. E. Elliot.
M.B., CH.B.—*R. C. Garry, ‡Catherine Farrower, ‡R. Crawford, ‡I. Grant, ‡A. T. Hastie, ‡Catharine Boyd, ‡T. W. Davidson, ‡R. C. Hamilton, ‡Mary P. MacCunn, ‡W. D. Hood, ‡A. Anderson, ‡J. W. Hamilton, ‡T. Nicol, ‡J. H. Ramage, ‡T. W. Howie, ‡J. Cook, A. W. Aird, Margaret Alexander, R. G. M. Alexander, Isabel P. Allen, A. Anderson, W. C. Andrew, Margaret R. Balloch, A. L. Bernstein, Catherine S. A. S. Blair, Mary MacQ. Bonnyman, Mary D. A. Boyd, F. A. Brown, R. O. Bruce, Barbara A. Cameron, Janet R. Campbell, Margaret E. Campbell, Margaret G. Carrick, Nora T. Cassidy, Gladys M. Chapell, H. Collingbourne, Margaret E. Colville, Elizabeth S. Cook, J. S. Cook, G. C. Cruijwagen, J. S. Currie, Isabella C. Darling, Agnes C. J. Davidson, Margaret Davidson, J. Dawson, Isabelle A. Deans, Agnes F. Dickson, Lilian M. Dickson, J. S. Donaldson, A. F. Dunn, Amy M. Fleming, Elizabeth H. Forrest, A. Fyfe, A. Gardiner, A. V. Garscadden, Emmie C. Gibb, T. Gibson, Jean M. Gilchrist, Margaret B. Graham, R. A. Grant, W. N. Gray, H. G. Halliday, Mary N. Hendry, G. J. Hutchison, Elizabeth G. Jamieson, Jessie M'L. C. Jamieson, Mary K. Jeffrey, T. J. Jones, F. C. Laing, J. E. S. Lee, A. Leitch, J. Lindsay, G. W. Lochhead, Isabella Lumsden, W. W. Lundie, H. M'Clusky Joan A. MacColl, J. W. M'Conville, A. A. Macdonald, Chrissiebelle Macdonald, T. J. M'Kail, K. W. MacKenzie, Marion A. M'Kenzie, G. MacKerracher, M. M'Lean, N. A. MacLean, S. M'Mahon, R. C. MacMurray, A. M'Nulty, Marion L. M'Quaker, Mary M'Quaker, Doris M'Walter, Agnes L. Mair, D. C. Marshall, D. V. Marshall, H. R. Melville, Elizabeth W. Miller, J. H. Miller, A. Mitchell, W. T. Mitchell, D. R. MacP. Morrison, Ellen D. Morton, W. Muir, J. P. Neilson, Sarah H. Nelson, Elizabeth K. Nicholson, J. Nicholson, J. L. Orr, J. A. R. Oswald, M. G. Pezaro, Louise B. Pollok, T. C. Porter, Mary E. Proudlove, T. D. Pyle, J. S. Ramage, Elizabeth C. Rodger, Bessie S. Ross, Mrs. Sarah Ross, J. Russell, Bertha E. A. Sharpe, W. Simpson, Christina R. I. Sloan, A. Snaddon, J. Sommerville, J. F. M. Stenhouse, Effie S. Stephen, Mary MacP. Stevenson, Jeanie M. Strathie, T. Tannahill, H. G. B. Teggart, R. W. Todd, Margaret S. Watt, Jemima Waugh, Janet A. O. Weir, A. F. Whyte, Catherine B. Wilson, J. H. Wilson, J. W. Wilson, J. H. Wright, Grace M. Young, J. A. K. Young.

Mr. R. C. Garry has gained the Brunton Memorial Prize of £10 awarded to the most distinguished graduate in medicine for the year.

*With honours. †With high commendation. ‡With commendation.

CONJOINT BOARD IN SCOTLAND.

THE following candidates have been approved at the examination indicated:

FINAL EXAMINATION.—*Medicine:* Beatrice M. Niven, C. J. B. Fox, Hilda Page, J. B. Rodrigo, Katharine M. Cellan-Jones, E. Brazao, J. M. Crombie, *Surgery:* C. J. B. Fox, W. Paris, J. B. Rodrigo, D. Maximos, J. M. Crombie. *Midwifery:* Beatrice M. Niven, El Saeed el Gohary Sheir, Don V. Walpola, Behari Lal Chopra, O. H. D. Oliver, E. Brazao. *Medical Jurisprudence:* R. F. Pagnam, W. J. Rankine, Annie H. Sutherland, W. D. Howat, L. L. Armstrong, A. H. Stephenson, D. Jack, G. A. P. McConey, J. Courtney, W. E. Haydon, C. J. Coventry, W. N. Stirling, E. Levine, J. Donnelly, J. Kirkness.

The following candidates having passed the Final Examination have been admitted L.R.C.P.E., L.R.C.S.E., L.R.F.P. and S.G.:

J. E. Overstead, A. V. Bergh, A. A. Dewar, S. I. T. Wills, Marjory J. A. Ogilvie, J. A. McE. S. Evers, P. Schneiderman, Lilian M. Williams, P. B. Fernando, W. O. Rodrigo, L. I. Myerson, S. J. Rom, J. M. McInnes, C. J. Cellan-Jones, W. L. Murray, Isabel F. King, T. T. Hoskins, F. H. A. Stegmann, S. Ponniah, P. J. B. Dyce.

The following candidates, having passed the requisite examinations, have been admitted Diplomates in Public Health:

D. J. A. Kerr, Jane W. Macdonald, Gertrude D. MacLaren, A. Climie, Jean MacI. Mackintosh, Mary Steven, Agnes H. M. Young, R. C. L. Batchelor, Annie M. Alexander, J. A. Fraser, A. Joe, J. H. Clarke, D. R. Cramb, R. J. O. Taylor, Mary L. Wilson, Christian M. Fleming, J. McCulloch, Eileen R. Freeman, Marion C. Taylor, J. McGarrity.

ROYAL COLLEGE OF SURGEONS OF ENGLAND.

A QUARTERLY Council was held on October 12th, when Sir Anthony Bowlby, President, was in the chair.

The late Sir Charles Ryall.—A vote of condolence was passed to the relatives of the late Sir Charles Ryall. The President reported that the vacancy on the Council so occasioned would be filled up at the meeting of Fellows in July, 1923.

Annual Report.—This report, to be presented to the Fellows and Members at the annual meeting on Thursday, November 16th, at 3 p.m., was approved and adopted.

Jenks Scholar.—Mr. Douglas Stanley-Jones was nominated as the thirteenth Jenks Scholar.

The Services.

TERRITORIAL DECORATION.

THE King has conferred the Territorial Decoration upon the following officers of the R.A.M.C., T.A., under the terms of the Royal Warrant, October 13th, 1920: Lieut.-Colonels: O. L. Appleton (ret.), G. N. Biggs (ret.), F. Hawthorn, D.S.O., J. W. Keay, A. Ogston. Majors: J. Allan, W. Duncan, O.B.E., A. H. Falkner, D.S.O., W. H. Fisher, O.B.E., R. B. Purves, A. B. Sloan (ret.), Captain E. M. Douglas-Morris.

Medical News.

SIR FREDERICK MOTT has accepted an invitation to become honorary director of the Mental Research Department newly established by a Research Board, appointed jointly by the Birmingham Corporation Asylums Committee and the Council of the University. It is the intention to appoint him also lecturer in neuropathology and psychotherapy in the University. The authorities of other mental hospitals in the Midlands have been invited to send representatives to the Board.

DR. JOHN M. T. FINNEY has been appointed temporary surgeon-in-chief of the Johns Hopkins Hospital and professor of surgery in the medical school, in place of the late Dr. W. S. Halsted. A meeting of the Faculty is being held this month, when a committee will be appointed to make a permanent selection.

A MEETING of the Medical Section of the British Psychological Society will be held at the Royal Society of Medicine on Wednesday, October 25th, at 8.30 p.m., when Dr. E. W. Scripture will read two papers, entitled "Some medical novelties in Vienna" and "Nature and treatment of epilepsy."

DR. THOMAS WATTS, an ex-president of the Ashton-under-Lyne Division of the British Medical Association, has been adopted by the Conservative party of the Withington Division of Manchester as prospective candidate at the next Parliamentary election.

THE Association of Alumni of the University of Bristol (London Branch) has arranged an inaugural dinner to be held at La Renommée Restaurant, 52, Dean Street, Shaftesbury Avenue, London, on Monday, November 6th, at 7.30 p.m. Lord Haldane is the president of this branch, and it is hoped that many members of the university, both past and present, will attend.

A MEMORIAL service for Dr. M. Mitchell Bird, late medical superintendent of St. Mary's Hospital (Paddington) for over thirty years, will be held in the chapel of the hospital on Tuesday, October 24th, at 2.30 p.m.

ON the suggestion of Dr. Arthur Hughes, county medical officer for Carmarthenshire, Dr. G. Arbour Stephens, of Swansea, has been appointed consultant for the investigation of the cause and incidence of heart disease among children. Dr. Stephens is already honorary cardiologist to the Welsh National Memorial Association.

A MEETING of the Harveian Society of London will be held at the Town Hall, Harrow Road, Paddington Green, on Thursday, October 26th, at 8.30 p.m.. when a discussion will take place on "The notification of infectious diseases," to be opened by Dr. Reginald Dudfield, followed by Dr. Charles Buttar, Dr. Menzies, Dr. Hamer, and Dr. Carnwath.

THE eighth Annual Report of the Board of Control, for 1921, has been issued this week. (H.M. Stationery Office, 7s. 6d. net.) The prefatory note describes the year as having been one of great administrative difficulty. Financial stringency compelled local authorities to check expenditure in every possible direction, and in lunacy no capital expenditure was authorized except in affairs of urgency. As a result many matters important for the treatment of the patients and the conditions under which they live had to be postponed. At the close of the year the number of notified insane persons under care in England and Wales was 123,714, an increase of 3,370 on that recorded at the beginning. The main factor in the increase was again the very low number of deaths in institutions for the insane. We hope to refer in more detail to the report in a later issue.

DR. F. L. THOMAS, honorary medical officer and ophthalmic surgeon to the North Devon Infirmary, has been elected mayor of Barnstaple.

FOUR lectures in physic will be delivered at Gresham College, Basinghall Street, E.C.2, on "The brain and mind: moral responsibility, criminal responsibility," by Sir Robert Armstrong-Jones, C.B.E., M.D., on Tuesday, Wednesday, Thursday, and Friday, October 24th, 25th, 26th, and 27th, at 6 p.m. Admission to the lectures is free.

A MEETING of the Society of Superintendents of Tuberculosis Institutions will be held at 122, Harley Street, W.1, on Friday, October 27th, at 4 p.m., when papers will be read by Sir Robert Jones on tuberculous disease of the knee, ankle and tarsus, and by Dr. Gordon Pugh on the treatment of tuberculous disease of the spine and hip-joint. There will also be a discussion on the curriculum for the training of nurses in tuberculosis.

THE formal opening of Langham Tower as the Sunderland Training College for Women Teachers took place on October 6th, in the presence of a large company, when Alderman Dr. R. Gordon Bell, chairman of the Sunderland Higher Education Subcommittee, declared the new premises open. On behalf of the staff and students, the Principal of the College presented to Dr. Gordon Bell the academic robes of his doctorate in medicine of Glasgow University. The Principal asked him to accept the gift as a memento of the occasion, and of their gratitude for the work he had done for higher education and in particular for the Training College.

THE number of undergraduates in residence at Oxford this term and the number of freshmen are both slightly lower than this time last year. The number of freshmen this year is 1,043, as compared with 1,073 in Michaelmas term 1921, and the total number of undergraduates is 3,687, compared with 3,928 last year. The number of new women students shows an increase from 225 last year to 263 this year; the total also is larger—722 as compared with 628.

THE second Winter School for Health Visitors organized by the Women Sanitary Inspectors' and Health Visitors' Association will be held in London from December 28th to January 11th. Lectures on various aspects of public health work will be given. Particulars may be obtained from the Secretary, Miss C. Macdonald, 5, York Buildings, Adelphi, W.C.2, on receipt of a stamped addressed envelope.

WE deeply regret to announce, as we go to press, the death, on October 18th, of Sir JAMES GALLOWAY, K.B.E., C.B., Senior Physician to Charing Cross Hospital. An obituary notice will appear in our next issue.

Letters, Notes, and Answers.

As, owing to printing difficulties, the JOURNAL *must be sent to press earlier than hitherto, it is essential that communications intended for the current issue should be received by the first post on Tuesday, and lengthy documents on Monday.*

ORIGINAL ARTICLES and LETTERS *forwarded for publication are understood to be offered to the* BRITISH MEDICAL JOURNAL *alone unless the contrary be stated.*

CORRESPONDENTS who wish notice to be taken of their communications should authenticate them with their names—of course not necessarily for publication.

AUTHORS desiring reprints of their articles published in the BRITISH MEDICAL JOURNAL are requested to communicate with the Office, 429, Strand, W.C.2, on receipt of proof.

IN order to avoid delay, it is particularly requested that ALL letters on the editorial business of the JOURNAL be addressed to the Editor at the Office of the JOURNAL.

THE postal address of the BRITISH MEDICAL ASSOCIATION and BRITISH MEDICAL JOURNAL is 429, Strand, London, W.C.2. The telegraphic addresses are:

1. EDITOR of the BRITISH MEDICAL JOURNAL, *Aitiology, Westrand, London*; telephone, 2630, Gerrard.
2. FINANCIAL SECRETARY AND BUSINESS MANAGER (Advertisements, etc.), *Articulate, Westrand, London*; telephone, 2630, Gerrard.
3. MEDICAL SECRETARY, *Medisecra, Westrand, London*; telephone, 2630, Gerrard. The address of the Irish Office of the British Medical Association is 16, South Frederick Street, Dublin (telegrams: *Bacillus, Dublin*; telephone, 4737, Dublin), and of the Scottish Office, 6, Rutland Square, Edinburgh (telegrams: *Associate, Edinburgh*; telephone, 4361, Central).

QUERIES AND ANSWERS.

INCOME TAX.

"G. H. S." has been refused an allowance for the cost of replacing a car on the ground that his replacements have been unnecessarily frequent.

*** This ground seems quite insufficient. "G. H. S." is in general practice, and is therefore assessed under Schedule D. He is entitled under the rules of that Schedule to deduct expenses incurred professionally whether necessary or not—that point applies only to assessments under Schedule E in respect of the remuneration of employment—for example, of a salary. Apart from this the objection taken is unfair, seeing that the more frequent the replacement the less, other things remaining the same, each individual replacement will cost. We suggest that the local inspector be asked to quote his legal authority for the refusal.

"H. S. B." has been refused deductions on account of expenses incurred in connexion with (1) Medical Defence Union, (2) a medical institute, (3) British Medical Association, (4) post-graduate course, (5) legal arrangements for partnership.

*** (1), (2), and (3) are allowable, or at least in the ratio in which the total subscriptions of each body for the year were actually expended on current work. (4) Not allowable—represents capital outlay. (5) Not allowable—it is an expense incurred in acquiring the partnership share and is therefore of a capital nature.

"NOVICE" has recently purchased a half-share of a practice.

*** 1. The assessment must be made on the amount of the earnings of the practice as a whole, and in dividing the duty payable between the partners no regard should be had to the fact that the cash receipts are not equally divided because portions relate to periods prior to the partnership. The cash receipts are taken as forming the basis for the calculation of the assessment for the current year, and it is on the earnings of that year that tax is being paid.

2. "Novice" can deduct tax at the standard rate from the payments of interest—for example, instead of paying £10 gross he should pay £10 less tax at 5s. in the £—that is, £7 10s.

3. On the facts stated no other deduction appears to be due.

"A. M. F." is being assessed to income tax in respect of untaxed interest received in the previous year, though the war loan stock has been so dealt with as to yield taxed instead of untaxed interest.

*** If "A. M. F." has now *no* income in the form of untaxed income he would not seem to be assessable. If he has received untaxed interest—even to a small amount—in this financial year he would be assessable on the amount of interest received in the previous year. Our correspondent will probably find that he did not pay tax for the first year he received the interest as it was not assessable until the second year.

SWEATING IN PULMONARY TUBERCULOSIS.

"IRISHMAN" asks for advice in the treatment of very profuse sweating in a case of pulmonary tuberculosis; the patient is 32; the area of lung involved is quite moderate, as also are the evening temperature and the amount of sputum; the hygienic conditions are superb, and two nurses are in attendance; the routine remedies are futile, and there is a definite history of hyperidrosis in the family; the only treatment for the tuberculosis followed is that of "nascent iodine."

LETTERS, NOTES, ETC.

TRICHINIASIS.

DRS. D. A. RICE and H. O. WILLIAMS (Milford Haven) write: In your issue of April 29th, 1922 (p. 680), you were good enough to allow us to describe certain symptoms occurring in a group of cases under our care. Judging by the correspondence we received our note was not without interest to many of your readers, and we shall be glad if you will allow us to state that the symptoms were those of trichiniasis, and not, as suggested, of an abdominal form of influenza. A full report of the outbreak appeared in last week's *Lancet*.

VACANCIES.

NOTIFICATIONS of offices vacant in universities, medical colleges, and of vacant resident and other appointments at hospitals, will be found at pages 34, 35, 38, 39, and 40 of our advertisement columns, and advertisements as to partnerships, assistantships, and locumtenencies at pages 36, 37, and 38.

A short summary of vacant posts notified in the advertisement columns appears in the *Supplement* at page *147*.

SCALE OF CHARGES FOR ADVERTISEMENTS IN THE BRITISH MEDICAL JOURNAL.

	£ s. d.
Six lines and under	0 9 0
Each additional line	0 1 6
Whole single column (three columns to page) ...	7 10 0
Half single column	3 15 0
Half page	10 0 0
Whole page	20 0 0

An average line contains six words.

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Advertisements should be delivered, addressed to the Manager, 429, Strand, London, not later than the first post on Tuesday morning preceding publication, and, if not paid for at the time, should be accompanied by a reference.

NOTE.—It is against the rules of the Post Office to receive *poste restante* letters addressed either in initials or numbers.